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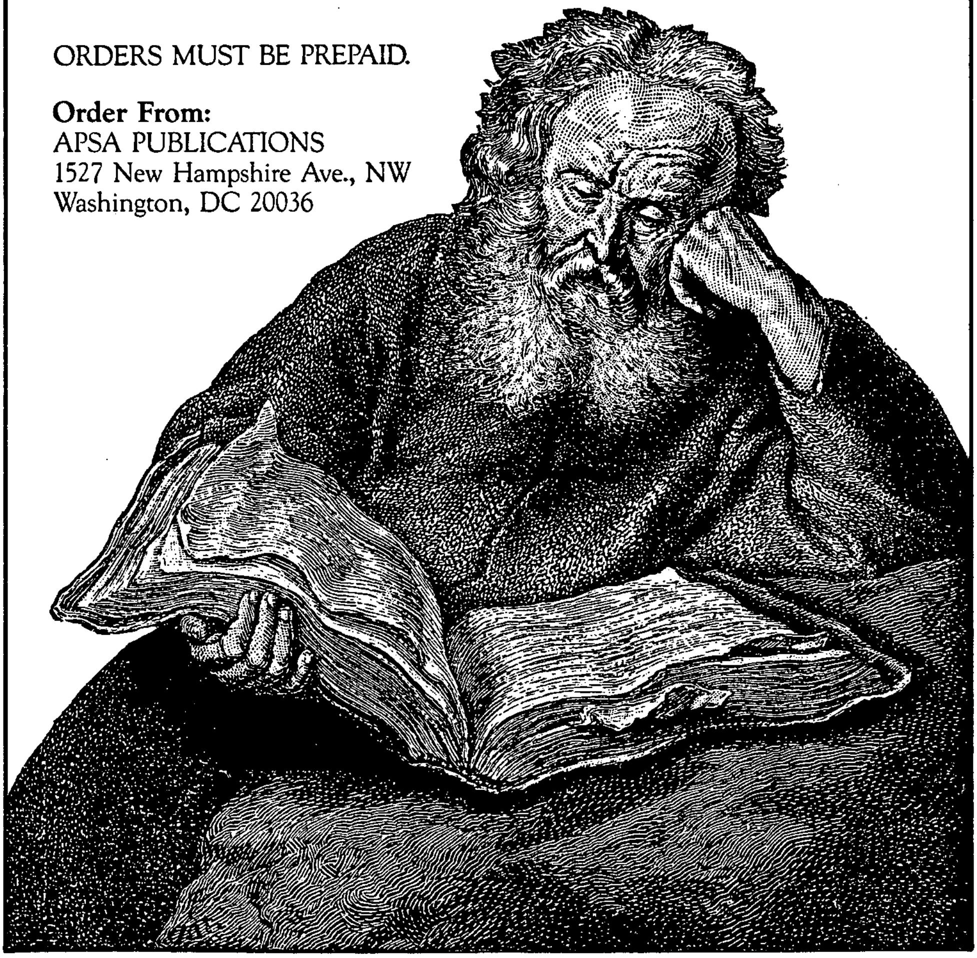

ARTICLES

POLITICS, MARKETS, AND THE ORGANIZATION OF SCHOOLS

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We offer a comparative analysis of public and private schools, presenting data from a new national study—the Administrator and Teacher Survey—that expands on the pathbreaking High School and Beyond survey. We find that public and private schools are distinctively different in environment and organization. Most importantly, private schools are more likely to possess the characteristics widely believed to produce effectiveness. We argue throughout that the differences across the sectors are anchored in the logic of politics and markets. This argument derives from our belief that environmental context has pervasive consequences for the organization and operation of all schools and specifically that the key differences between public and private environments—and thus between public and private schools—derive from their characteristic methods of social control: the public schools are subordinates in a hierarchic system of democratic politics, whereas private schools are largely autonomous actors "controlled" by the market.

Virtually all public schools in the United States are governed by democratic institutions of the same basic form. This form is now taken for granted. There is a broad consensus that democratic control of the public schools is a good thing and that democratic control means control through local school boards, superintendents, central office bureaucracies, and corresponding apparatuses at the state and (increasingly) the federal levels. However heated the conflict over educational policy and practice, however intense the struggle for influence and resources, the "one best system" stands above it all (Tyack 1974).

In recent years, educational politics has centered on the quality of the public schools. Long-simmering discontent about declining test scores, loose academic standards, and lax discipline—fueled by a series of national studies—has engendered a widespread reaction against the "rising tide of mediocrity" (National Commission on Excellence in Education 1983), and state legislatures around the country have responded with reforms ranging from stricter academic requirements to merit pay plans for teachers (Doyle and Hartle 1985). Throughout this period, the "one best system" has provided the institutional framework within which problems have been identified and policy responses chosen. It structures criticism and reform, but it is never their target.

Much the same is true within educational research, which has generally taken institutions as given. Studies of school effectiveness have focused directly on the schools, asking about those aspects of organization and immediate environment that explain school performance. Taken as a whole, this work has promoted a loose consensus on factors that appear to enhance effectiveness, among them, clear school goals, rigorous academic requirements, an orderly climate, strong instructional leadership by the principal, teacher participation in decision making, cooperative principal-teacher relations, active parental involvement, and high expectations for student performance (Boyer 1983; Brookover et al. 1979; Goodlad 1984; Powell, Farrar, and Cohen 1985; Rutter et al. 1979; Sizer 1984).

This research has shaped the contours of public debate by suggesting traits good schools ought to have. But it is the institutional system itself, accepted by one and all, that tells us how these desirable features are to be transmitted to the schools: they are to be imposed from above. For many objectives—tougher academic requirements, say—reform simply calls for new legislative or district policy. Not coincidentally, these have been among the more popular reforms. Other objectives are less amenable to formal imposition—cooperative relations within the school, for example. But these tend to be regarded as matters of good management and training, and thus as reforms that can be delegated to the professional side of the control structure. Whether the means are formal or professional, then, the rationale of democratic control is to "make" schools more effective by imposing desirable traits on them (Campbell et al. 1985).

These reforms are likely to fail. To see why, it is useful to begin with a curious feature of the way schools are conventionally understood. Among those who study education, it is received doctrine that schools are open systems and thus products of their environments (Scott and Meyer 1984; Weick 1976). By this logic it should follow that the organization and performance of schools are largely explainable by the environments that surround them. Different types of environments should tend to produce different types of schools. When schools turn out to have undesirable characteristics, the logical culprit is the environment—not the schools.

Yet studies of school effectiveness have rarely taken the environment seriously. They tend to explain poor performance in terms of variables inside and immediately outside the school—and then they turn to our institutions of democratic control to make the necessary changes. Our institutions, however, are core components of the very environment that by open systems reasoning is likely to have caused the problems in the first place. These studies should be asking, What is the relationship between democratic control and the organization of schools? Might there be something inherent in these institutions that systematically promotes organizations of a type no one really wants?

We do not pretend to have all the answers. We do think, however, that institutions are fundamental to an understanding of schools. Here we try to make a plausible case for this view by developing a theoretical argument and presenting some new evidence from a recent survey.

Our basic argument is that the organization of schools is largely endogenous to the system of institutional control in which the schools are embedded. Different systems of institutional control should tend to produce schools with distinctive patterns of characteristics. While we will specify these patterns in some detail, our most general claim is simply that the hierarchy of democratic control, the "one best system," puts its stamp on the organization of our public schools and that this stamp holds the key to school quality and school reform.

Institutional issues are often difficult to explore through empirical research. This is especially true in studying the "one best system." How can we study institutional effects if there is only one, all-encompassing institution? An instructive way to proceed, we believe, is to compare public schools to those schools that fall outside the hierarchy of democratic control: private schools.

That is what we do here. We explore the logic of institutional control in the two sectors and derive implications for schools. This line of reasoning suggests that schools should indeed look different across sectors and, most importantly, that democratic control should inhibit the emergence of "effective school" characteristics. Using data obtained from a representative sample of public and private high schools, we compare them on a range of characteristics commonly associated with effective academic performance. These results consistently suggest that institutions are important determinants of school organization—and that, in consequence, public schools are quite literally at a systematic disadvantage.

Politics, Markets, and Control

Public schools are controlled by democratic authority and administration. The specifics vary from district to district and state to state, but the basic framework is remarkably uniform throughout the country. The private sector might seem to lack any comparable uniformity. Most private schools are affiliated with a church; some are elite preparatory schools; some are military academies; and there are other types as well (Kraushaar 1972). But they all have two important institutional features in common: society does not control them directly through democratic politics, and society does control them—indirectly—through the marketplace. Rather than marvel at diversity, we find it useful to think about schools in terms of these alternative institutions of social control. As a shorthand, we will refer to them as politics and markets.

We want to provide a little background on how these institutions operate and what they imply for schools. We will not yet relate them to specific aspects of school organization. This will be done later when we turn to a discussion of the survey data.

Constituents and Consumers

Popular myth lauds the role of local citizens and their elected school boards in governing the public schools. But the fact is that the schools are not locally controlled and are not supposed to be. The state and federal governments have legitimate roles to play in financing schools, setting standards, and otherwise imposing their own policies. This means that U.S. citizens everywhere, whether or not they have children in school and whether or not they live in the district or even the state, have a legitimate hand in governing each school (Campbell et al. 1985; Wirt and Kirst 1982).

The proper constituency of even a single public school is a huge and heterogeneous one whose interests are variously represented by formally prescribed agents—politicians and administrators—at all levels of government. Parents and students, therefore, are but a small part of the legitimate constituency of "their own" schools. The schools are not meant to be theirs to control and are literally not supposed to provide the kind of education they might want. Public education is shaped by larger social purposes as defined by larger constituencies.

Private schools determine their own goals, standards, and methods. These may reflect the values of owners or patrons, or perhaps a collective such as a diocese. But the market imposes a fundamental constraint. Private schools provide services in exchange for payment, and unless heavily subsidized from the outside, they must please their consumers—students and parents—if they are to prosper. Whatever the constituency of the private school, therefore, it will surely be much smaller and more homogeneous than the democratic constituency of the public school, and students and parents will occupy a much more central position within it.

Exit and Voice

In the private marketplace, educational choice is founded on what has come to be called, following Hirschman (1970), the *exit* option. If parents and students do not like the services they are being provided, they can exit and find another school whose offerings are more congruent with their needs. This process of selection promotes a match between what educational consumers want and what their schools supply. Matching is reinforced by the population effects (Alchian 1950) of selection: schools that fail to satisfy a sufficiently large clientele will be weeded out (or, if subsidized, become an increasing burden).

Selection also forges a strong bond between consumer satisfaction and organizational well-being. This gives schools incentives to please their clientele, as well as to set up *voice* mechanisms—committees, associations—that build a capacity for responsiveness into organizational structure. These incentives, too, promote a match—but they are not necessary for success. A school might rigidly adhere to purist doctrine yet succeed because that is what enough consumers happen to want. Either way, the result tends to be the same: a match.

In the public sector, popular control is built around voice. Exit plays a minimal role. The public school is usually a local monopoly, in the sense that all children living in a given area are assigned to a particular school. This does not eliminate choice, since parents can take account of school quality in deciding where to live. But residential decisions involve many factors in addition to education, and once they are made, sunk costs are high.[1] Low or declining quality need not keep parents from moving into an area, and it is even less likely to prompt existing residents to pick up and leave.

It might prompt them to consider a private school. But here they confront a major disincentive: public schools are free, private schools are not. Due to this cost differential, the perceived value of private schools must far outweigh that of public schools if they are to win students. To put it the other way round, public schools, because they are relatively inexpensive, can attract students without seeming to be particularly good at educating them.

Lacking a real exit option, many parents and students will choose a public school despite dissatisfaction with its goals, methods, or personnel. Having done so, they have a right to voice their preferences through the democratic control structure—but everyone else has the same rights, and many are well armed and organized. Voice cannot remedy the mismatch between what parents and students want and what schools provide. Conflict and disharmony are built into the system.

Autonomy and Control

In the private sector, the exit option not only promotes harmony and responsiveness, it also promotes school autonomy. This is true even for schools that are part of a hierarchy, as the Catholic schools are. The reason is that most of the technology and resources needed to please clients are inherently present at the bottom of the hierarchy—in the school—since educational services are based on personal relationships and interactions, on continual feedback, and on the knowledge, skills, and experience of teachers. The school is thus in the best position to know how to enhance its own organizational well-being. Hierarchical control, or any external imposition, tends to be inefficient and counterproductive.[2]

Central direction is important when superiors have an agenda of their own that cannot be pursued simply by pleasing clients. In the private sector, imposition of such an agenda involves a trade-off: if schools are constrained in their efforts to please clients, dissatisfied clients can leave. In some hierarchies—notably, those associated with churches—superiors may consider this an acceptable price; they may prefer "pure" schools to growing, prosperous ones. But it is still a price, one that threatens organizational well-being—and one that in the limit can be fatal. Thus, even if there are higher-order values to be pursued, the exit option discourages tight external control in favor of school autonomy.

In the public sector the institutional forces work in the opposite direction. The raison d'être of democratic control is to impose higher-order values on schools and thus limit their autonomy. Exit is an obstacle to control: when the governance structure imposes a policy on parents and students who disagree, exit allows them to avoid compliance by "voting with their feet," thus defeating the purpose of the policy. But public officials do not have to take exit as a given. They can simply pass laws restricting its availability. While private decision makers value autonomy because it helps them cope with problems of exit, public officials eliminate exit in order to facilitate their imposition of higher-order values.

The drive to restrict autonomy is built into the incentive structures of politicians and bureaucrats. Politicians seek political support by responding to various constituency groups, particularly those that are well organized and active. These include teachers' unions and associations of administrators, but also a vast array of groups representing more specialized interests—those of minorities, the handicapped, bilingual education, drivers' education, schools of education, book publishers, and accrediting and testing organizations, among others. These groups typically have financial or occupational stakes in existing educational arrangements, and their policy positions reflect as much. They all want a share of the public's educational resources. They want to influence educational programs. They want to have a say in how the schools are organized and operated. And politicians are only too happy to oblige—this is the path to political popularity (Masters, Salisbury, and Eliot 1964; Iannaccone 1967; Peterson 1976; Wirt and Kirst 1982).

Bureaucrats play both sides of the governmental fence. Their power rests on the fact that bureaucracy is essential to direct democratic control. The imposition of higher-order values is hardly automatic, particularly given the built-in dissatisfaction of parents and students and the inevitable pressures from teachers and principals for autonomy. Control requires rules and regulations, monitoring, incentive structures, and other means of ensuring that those engaged in the educational process behave as they are supposed to behave. It requires bureaucracy—and bureaucrats.[3]

As public officials they have incentives to expand their budgets, programs, and administrative controls. These are the basics of bureaucratic well-being, and their pursuit is an integral part of the job. But bureaucrats also belong to important interest groups—of administrators, of professionals—that lobby government from the outside (ostensibly) as well. Although traditionally they have portrayed themselves as nonpolitical experts pursuing the greater good, they are in fact a powerful special interest—an interest dedicated to hierarchical control (Knott and Miller 1987; Tyack 1974).

The system, in short, is inherently destructive of autonomy. Politicians have the authority to shape the schools through public policy, and, precisely because they have this authority, they are consistently under pressure from interest groups to exercise it. It is in their own best interests to impose choices on the schools. The same is true of bureaucrats, who have occupational and professional stakes in control: a world of autonomous schools would be a world without educational bureaucrats. Thus, while principals and teachers may praise the virtues of autonomy, the "one best system" is organized against it. Politicians, bureaucrats, and virtually the full spectrum of interest groups tend to see autonomy for what it is: a transfer of power and a threat to their interests.[4]

Purpose and Performance

Public schools are products of public policy. With a huge constituency, there is inevitably dissension over what constitutes "good" policy—and many of the contending groups have their own stakes in public education and are not simply struggling to provide us with "good" policy anyway. Even if they were, there is no guarantee they could implement it very effectively, for bureaucratic control is an inherently difficult and costly means of engineering educational outcomes.

Reform grows naturally out of all this. When important groups signal their dissatisfaction with what the schools are doing, politicians and bureaucrats spring into action. They respond to group demands by doing what they are institutionally empowered and motivated to do: they seek remedies through new policies and new controls. This is the characteristic way in which the public schools are "improved." And because administrative problems, value conflicts, and shifts in power alignments are endemic to the system, reform is a never-ending process.

Private schools operate in a wholly different institutional environment. Reform occurs when schools find it in their own best interests to make adaptive adjustments, when new schools enter the educational marketplace, and when unpopular schools fail. All are closely tied to the interests of parents and students.

Are private schools also likely to be better than public schools? In an important sense, the answer is *yes*. Parents and students who choose a private school are revealing their judgment of quality: the private school is not only better, it is better by an amount that exceeds the cost differential. Since there is clearly an objective basis for their judgment—they directly experience private education and are free to return to the public sector at any time—we have good reason to believe that private schools are in fact more effective at providing the types of educational services their clients care about.

But are they better at the important things schools ought to be doing? This question cannot be answered without substituting our value judgments for those of parents and students. A church school that attracts students on the basis of religious and moral training almost surely outperforms the local public school on this dimension. But this says nothing about their relative effectiveness in transmitting democratic values or an appreciation of cultural diversity. Performance is only desirable if the goals are desirable.

This, of course, is the justification for democratic control. In principle, our institutions are set up to articulate important social goals and to ensure that schools act effectively on them. If private schools do a better job of providing certain services or of pleasing parents and students, this does not mean that society must therefore prefer private to public education. Any evaluation has to depend on a more fundamental judgment about what the schools ought to be doing.

In objective terms the two institutional systems are simply very different, and they give rise to schools that reflect these differences—providing different services in different ways to please different constituencies.

Data and Method

High School and Beyond (HSB), first administered in 1980, is the most comprehensive survey of secondary schools to date. The original data base, pertaining to some 60 thousand students in more than one thousand public and private schools, provided a rich source of information about student achievement, attitudes, activities, and family background. This was the empirical foundation for Coleman, Hoffer, and Kilgore's (1982) *High School Achievement*, which set off shock waves in the educational community with its conclusion that private schools are academically more effective than public schools.[5]

High School and Beyond included certain information about the schools, but important aspects of organization and environment were not part of the study. To augment the data base, we helped design the Administrator and Teacher Survey (ATS), which went back to about five hundred HSB schools and administered questionnaires to the principal, a sample of 30 teachers, and selected staff members in each. Their responses tell us a good deal more about the schools as organizations—about their external relationships, their leadership, their structure and goals, their patterns of influence and interaction, and their educational practices. We put the ATS data to use in exploring how education is organized in the public and private sectors.[6]

Because private schools are so diverse, empirical work on student achievement has frequently clarified sectoral comparisons by distinguishing two relatively homogeneous types of private schools in the HSB sample—Catholic and elite—from all the rest. Catholic schools have played the central role in these analyses. They are the majority of private schools, and their students are very similar on socioeconomic and ethnic grounds to students in the public sector. The elite schools are the handful of top private schools in the nation as judged by the proportion of seniors who were semifinalists in the National Merit Scholarship competition. The remaining schools, "other private," vary from tiny religious groupings to large college prep schools.[7] We will maintain these distinctions—and despite their marked diversity, we will expect a uniformity across the three types. For by virtue of their shared institutional context, they should give evidence of something that approaches a common syndrome of organization.

In the analysis, we simply regress each organizational or environmental characteristic against dummy variables representing the three types of private schools.[8] Specifically, if C is the characteristic in question, we estimate the following equation:

$$\begin{aligned} C = {} & B0 + B1\ \text{Catholic} \\ & + B2\ \text{other private} + B3\ \text{elite} \\ & + \text{error}. \end{aligned}$$

The constant, B0, measures the public school mean on C. *Catholic, other private*, and *elite* are dummy variables taking on the value 1 if the school is of that type, 0 otherwise. B1 measures the difference between the Catholic school mean and the public school mean on characteristic C. B2 and B3 measure the same private-public comparison for the other private and elite categories.

These comparisons are made without the usual laundry list of statistical controls. It is an easy matter to include controls for school size, student background, and countless other factors, but we think it would be inappropriate and possibly very misleading to do so at this point. In estimating relationships among variables, the purpose of statistical controls is to remove covariation due to prior or exogenous influences. An institutional perspective on the organization of schools, however, suggests that all major variables are probably endogenous.

It may be, for instance, that private schools are more likely to exhibit certain characteristics—happy teachers, perhaps—because they tend to be smaller organizations than public schools. But it is no accident that private schools tend to be smaller, since small size is a major basis on which they appeal to students, parents, and teachers. Small size and happy teachers are integral parts of the same syndrome. Similarly, private schools may seem to have desirable organizational traits—more orderly climates, say—because their students come from families that care more about education. But these families have chosen their schools in the first place precisely because they like the way the schools are organized. So what causes what? Do motivated students make for a good school, or do good schools attract motivated students? The most reasonable view is that causality flows both ways and thus that both variables are endogenous.

Analogous arguments apply for virtually any variables of interest. To control for them as though they are exogenous is to remove from the sectoral comparisons—and to remove in a methodologically inappropriate way (via additive terms in recursive equations)—factors that are integrally woven into the very fabric of each system. For now, prudence argues for getting a clear look at how basic aspects of environment and organization differ across sectors. Investigation of the causal structure—and with it, informed thinking about statistical controls—can proceed most usefully once this sort of foundation has been laid.

The Findings

External Authorities

If the operation of politics and markets suggests anything, it is that the control of schools should differ systematically across sectors. Public schools should find themselves operating in larger, more complex governing systems that tend to exert greater influence over school policy, educational practice, and personnel decisions. Private schools should tend to enjoy more autonomy with respect to their structure, goals, and operation. Of course, these tendencies are well documented when they derive from higher levels of government authority: public schools are part of state and federal hierarchies, integrated financially and programmatically, while private schools generally are not (Coleman, Hoffer, and Kilgore 1982). But what about immediate outside authorities? What kind of governing system operates at the local level to distinguish the sectors?

Table 1 presents summary figures on the extent to which the various schools are hierarchically subordinate to school boards or to outside administrative superiors (in the form of a superintendent or central office of some kind). Not surprisingly, virtually all public schools in the sample are governed by both; only two of nearly three hundred schools depart from this pattern. The private sector is far more diverse. Almost all private schools, regardless of type, have a school board of some sort, but often there is no accompanying administrative apparatus. Such an apparatus is quite rare among the elite schools, and nearly half of the "other privates" are similarly unencumbered. It is the Catholic schools that most resemble the publics in this regard, with some two-thirds of Catholic schools having both school boards and administrative superiors; even here, however, there is a good deal of hierarchic diversity by comparison to the public sector. Fully a quarter of the Catholic schools are overseen only by a school board, the Church's reputation for hierarchy notwithstanding.

Because administrative authorities are often absent from the environments of

Table 1. Types of Outside Authorities (%)

Outside Authorities Present	Public[b]	Catholic[c]	Other Private	Elite Private
School board and administrators[a]	99.3 (287)	69.0 (20)	52.9 (9)	28.6 (2)
School board only	.0 (0)	24.1 (7)	41.2 (7)	57.1 (4)
Administrators only	.3 (1)	3.4 (1)	.0 (0)	.0 (0)
None	.3 (1)	3.4 (1)	5.9 (1)	14.3 (1)
Total	99.9 (289)	99.9 (29)	100 (17)	100 (7)

Note: Numbers in parentheses are the unweighted number of schools in each sector.

[a] Administrators include superintendent or central office.

[b] Excludes several special public schools oversampled in ATS.

[c] Excludes exclusively black Catholic schools oversampled in ATS.

private schools, many of these schools will operate relatively free from bureaucratic control. But what about the control exercised by the political and administrative authorities that public and private schools often have in common? In Table 2 control by school boards as perceived by principals is compared along five basic policy dimensions: curriculum, instructional methods, discipline, hiring, and firing. The results are striking in their consistency. On all five dimensions school boards in the public sector appear to have more influence over school policy than they do in the private sector, regardless of the type of private school. The differences between public and private are consistently greater (and statistically significant) for personnel and disciplinary policy than for matters pertaining to educational practice and content.[9] They amount on the average to between one and two points on a six-point influence scale. But in view of the uniformity of the overall pattern, there is reason to suspect that even the small estimated differences for curriculum and instruction are indicative of a greater role by school boards in the public sector

Table 2. The Influence of School Boards on School Policies

Areas of Influence	Catholic	Other Private	Elite Private
School board			
Curriculum	-.27 (1.33)	-.24 (1.52)	-.07 (.04)
Instruction	-.03 (.14)	-.003 (.02)	-.03 (.01)
Discipline	-.80 (4.02)	-.35 (2.24)	-1.16 (.56)
Hiring	-.98 (5.16)	-.80 (5.31)	-1.11 (.56)
Firing	-.76 (3.83)	-.41 (2.63)	-.86 (.42)
School board vs. principal			
Curriculum	-.40 (1.96)	-.26 (1.63)	.05 (.03)
Instruction	-.06 (.31)	.41 (2.50)	.17 (.08)
Discipline	-.72 (3.59)	-.28 (1.75)	-.92 (.44)
Hiring	-.97 (4.89)	-.42 (2.67)	-1.30 (.63)
Firing	-1.08 (5.65)	-.72 (4.72)	-1.23 (.62)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

Table 3. The Influence of Administration on School Policies

Areas of Influence	Catholic
Administration	
Curriculum	-1.51 (8.19)
Instruction	-1.01 (5.12)
Discipline	-1.23 (6.39)
Hiring	-1.82 (10.28)
Firing	-2.11 (12.65)
Administration vs. principal	
Curriculum	-1.57 (8.44)
Instruction	-1.23 (6.33)
Discipline	-1.11 (5.66)
Hiring	-1.56 (8.39)
Firing	-2.00 (11.57)

Note: Administration includes superintendent or central office. Table reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

generally. This is reinforced by the perceived influence of school boards relative to principals. As outlined in the lower half of Table 2, the sectoral differences are repeated and sometimes amplified. Relative to their school boards, private principals play a more autonomous role in setting and implementing policy—especially as it pertains to personnel and discipline—than public principals do.

Relative to their administrative superiors, private principals appear to be similarly autonomous. At least this is true of principals in Catholic schools, the only type of private school with enough administrative supervision to make a comparison with public schools valid and instructive. The figures, presented in Table 3, suggest an interesting conclusion: that the famed Catholic hierarchy (although see Greeley 1977) plays a comparatively small role in governing Catholic schools. On all five dimensions, the influence of administrative superiors is far less in Catholic than in public schools. These differences are again greatest in the area of personnel policy, but here the other policy areas reflect substantial differences as well. When we explore the school's autonomy a bit further by comparing the principal's influence to that of administrative superiors, the same pattern emerges. Relative to administrators, Catholic principals enjoy more freedom than public principals in setting school policy.

To be sure, the differences in school autonomy that seem to distinguish the public and private sectors are based on simple measures of perceived influence and not on actual behavior. But the patterns these measures yield are quite uniform and entirely consistent with our expectations for external control. The authorities that are so ubiquitous in the democratic context of the public school are often simply absent from private school settings—and even when they are an acknowledged part of the private governing apparatus, they play less influential roles in the actual determination of school policy. Private schools, it would appear, have more control over their own destinies.

External Constraints: Choosing the Organization's Staff

Among the controls that any organization seeks to exercise over its operations, perhaps none is as important as control over its staff—in the case of a school, its teachers. To what extent does the school have flexibility in recruiting the kinds of teachers it wants and getting rid of those who do not live up to its standards? We have already seen that public schools are at a disadvantage in this regard, for external authorities have much more influence over hiring and firing in the public sector than they do in the private sector. The sectoral differences are not limited to the role of external authorities, however. They become still more dramatic when we consider two additional constraints on the choice of personnel: tenure and unions.

Tenure systems in public schools are special cases of the civil service systems that exist at all levels of government. Historically, these systems arose to prevent politicians from rewarding their supporters with public jobs. Reformers recognized that the widespread use of patronage was inconsistent with the kind of expertise, professionalism, and continuity so necessary to effective government, and —in a halting process that took decades to accomplish—they brought about the pervasive adoption of civil service systems built around objective qualifications and designed to protect employees judged to be qualified. Tenure is one of these protections (Peterson 1985).

Teacher unions (or "associations"), although initially resisted by politicians wedded to patronage, eventually found political allies of their own. Organized teachers could offer money, manpower, and votes to politicians. In state and local elections, where turnout is typically very low, these are attractive inducements indeed. As teacher unions thrived, they gained not only economic concessions but also contractual guarantees of job security and other limitations on responsibilities that reinforced the protections of the civil service system and introduced wholly new constraints into personnel decisions affecting the local school (Grimshaw 1979).

Although there is nothing to prevent unions from gaining a foothold in private schools nor to keep private schools from adopting tenure and other civil service-like protections, there is nothing comparable to government that drives them in that direction. Whether unions and tenure systems take hold in the private sector is determined much less by politics and much more by markets. Schools may choose to offer tenure and other protections as a means of attracting good teachers, particularly given that public schools offer that benefit. But private schools may also decide, especially if the supply of teachers is high, that they can offer a very attractive set of benefits—such as good students, orderly atmosphere, and collegial decision making—without offering tenure at all. Similarly, as in any market setting, unions may or may not succeed in organizing teachers. But they cannot count on symbiotic relationships with the authorities, as public unions can, to help their cause.

The ATS data suggest that the public and private sectors are in fact enormously different in these respects. While 88% of public schools offer tenure, only a minority of the private schools do: 24% of the Catholics, 39% of the elites, and 17% of the "other privates." Among the schools that do offer tenure, moreover, the proportion of teachers who have actually been awarded it reflects the same asymmetry: 80% of the eligibles in public schools have tenure, while the figure is some 10%-16% lower in the private sector. The differences in unionization are even more substantial. The vast majority of public schools are unionized—some 80%—almost all of them by either the National Education Association or the American Federation of Teachers. In the private sector, by contrast, teachers are rarely represented by unions. Only about 10% of the Catholic schools are unionized, and virtually none of the elites and "other privates" are.

To assess whether school control over personnel is perceptibly constrained by tenure, unions, and other proximate external authorities, we asked principals to evaluate an assortment of potential barriers to hiring excellent teachers and firing incompetent ones. On the hiring side, principals in the two sectors agreed on the severity of several obstacles, including applicant shortages and low pay. But public school principals were far more likely to complain about obstacles administrative in origin: "central office con-

Table 4. School Personnel Policy and Process

Personnel Constraints	Catholic	Other Private	Elite Private
Barriers to Hiring			
Too many transfers	-.57 (2.72)	-.56 (3.36)	-.43 (.20)
Central office control	-.51 (2.43)	-.25 (1.51)	-.47 (.22)
Barriers to Firing			
Complex procedures	-.59 (3.35)	-1.47 (10.51)	-1.26 (.69)
Tenure rules	-.93 (5.08)	-1.36 (9.39)	-1.55 (.82)
Hours involved in firing someone	-.85 (4.01)	-.75 (4.47)	-.90 (.41)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

trol" and "excessive transfers from other schools" (see Table 4).

The obstacles to dismissing teachers for poor performance differ similarly. In the public schools the procedures are far more complex, the tenure rules more constraining, and the preparation and documentation process roughly three times as long (Table 4). The complexity and formality of dismissal procedures is the highest barrier to firing cited by public school principals. For private school principals, of every type, the highest barrier is "a personal reluctance to fire." These responses provide a rather poignant statement of the differences between the sectors: while the public school principal is bound most by red tape, the private school principal is bound most by his or her conscience.

Principals do, of course, have other forms of control over their staffs. They can encourage undesirable staff to resign, retire, or transfer. They can offer good teachers special assignments or relieve them of onerous duties. They can recognize high performance with awards.[10] But none of these practices differs systematically across the sectors. Public principals simply have less power than private principals to mold and manage their teaching staffs.

Even if public superintendents or central offices wanted to delegate such power to the school—and, in general, there is no reason to think they have incentives to do this—many personnel decisions cannot in practice be delegated. Tenure protections are usually guaranteed through laws that are written by school boards or state legislatures, and these laws are then enforced by administrators. Union contracts are typically bargained at the district level, not at the school level, and are enforced from above. Tenure and unionization tend to settle the question of where and how the basic personnel decisions will be made in the public sector. They will be centralized. Schools in the private sector, largely free of such constraints, have far greater flexibility to choose their own members and chart their own paths.

Parents

In most respects, private schools would seem to have ideal parental environments. Parents, after all, have made a positive choice to send their children to a private school, presumably because they care about education and have a high appraisal of the school. And if at any time they change their views, they can simply exercise their exit option. This means that the school is likely to enjoy significant gains: they gain children whose family lives encourage education, and parents

Table 5. Parental Relationships with Schools

Parental Role	Catholic	Other Private	Elite Private
Monitoring students	.90 (4.69)	.43 (2.82)	1.30 (.65)
Expectations of students	1.24 (6.85)	.91 (6.33)	2.62 (1.39)
Involvement in school	.74 (3.81)	.52 (3.39)	.64 (.32)
Cooperativeness	.43 (2.13)	.18 (1.14)	.47 (.22)
Freedom from constraint	.57 (2.81)	.22 (1.35)	.49 (.23)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

who not only will facilitate school objectives by monitoring homework and the like but will be informed and supportive when they take an active interest in school decision making. Parents who may cause problems on these scores are precisely the ones most likely to drop out of the school's environment voluntarily.

Public schools are not so fortunate. Many of their students come from families that put little or no emphasis on education; the students come to school with poor attitudes and orientations, and the parents do little to facilitate the school's efforts. Because exit is often not a viable option, many parents who do not support the school's goals, methods, or activities will remain in its environment nevertheless; and some—perhaps many—will use the democratic mechanisms at their disposal, as well as interactions with principal and staff, to express their dissent and press for change. Far from gaining sustenance from a supportive parental environment, the public school may often find itself dealing as best it can with conflict, disappointment, and apathy.

Not all public and private schools will neatly fit these molds, of course. But it seems clear that characteristics inherent in the two sectors—characteristics anchored in politics and markets—encourage the kinds of environmental differences outlined here. And results from the ATS study, detailed in Table 5, are consistent with this line of reasoning. Parents in the private sector, regardless of the type of school, are uniformly more supportive of their schools. They have higher expectations about their children's educational performance, they are more active in monitoring their children's behavior outside of school, and they are more deeply involved with the school as an organization. Private school principals, not surprisingly, also express greater satisfaction with their parental environments. Relationships with parents are more cooperative than in the public sector, and they are less constrained by the kinds of formal rules and norms that due to democratic governing structures impinge on the flexibility of public school principals.

The operation of politics and markets, then, appears to put public schools at a real disadvantage. Because of forces largely beyond the control of the individual school, parents in the public sector tend to be less supportive of the school's general educational efforts and more likely to promote organizational conflict—and, to make matters worse, the school has less flexibility in seeking solutions to these problems. By comparison, private schools have fewer such problems and yet more flexibility for dealing with them.

Between Environment and Organization: The Principal

The principal operates at the boundary of the organization and is, more than any other single person, responsible for negotiating successfully with the environment

Table 6. Characteristics of School Principals

Characteristics	Catholic	Other Private	Elite Private
Teaching experience	.43 (2.21)	.56 (3.59)	.58 (.28)
Motivations			
Policy control	.61 (3.04)	.46 (2.92)	.31 (.15)
Preference for administrative duties	-.33 (1.63)	-.49 (3.08)	-.14 (.07)
Career advancement	-.69 (3.52)	-.61 (3.91)	-.48 (.23)
Desire further advancement	-.76 (3.95)	-.71 (4.69)	-1.04 (.52)
Leadership as perceived bv teachers	.41 (2.12)	.74 (4.84)	.64 (.32)
Instructional leadership	.66 (3.67)	1.28 (8.98)	.82 (.44)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

—responding to demands and pressures from parents, unions, administrators, and school boards, and dealing with external disruptions such as budget cuts, policy conflicts, and demographic changes. The principal may also hold a key to school effectiveness. Evidence increasingly suggests that educational excellence is promoted by a principal who articulates clear goals, holds high expectations of students and teachers, exercises strong instructional leadership, steers clear of administrative burdens, and effectively extracts resources from the environment (e.g., Blumberg and Greenfield 1980; Brookover et al. 1979; Goodlad 1984).

It is seldom stressed, however, that the school environment can have a lot to say about whether the principal is able to practice these precepts of effective leadership—or, for that matter, is even motivated to practice them. Effective leadership does not simply inhere in the individual filling the role; it is unavoidably contingent upon the demands, constraints, and resources that the principal must deal with. Depending on the nature and strength of these forces, even the "best" principal may have only a marginal effect on school performance. We must also remember that principals do not possess or lack leadership qualities by accident. Both environment and organization tend to ensure that there will be selective attraction to the job: certain schools will tend to attract certain kinds of principals. Similarly, principals will be socialized on the job, and internal and external factors will ensure that principals at distinctly different schools will be socialized differently. Thus, while it is one thing to point to certain qualities that appear conducive to effective leadership, it is quite another to suggest that principals are free to develop them.

It should not be surprising, then, to find differences between public and private school principals, both in terms of their own characteristics and in terms of their performance. Consider first what the ATS data (see Table 6) have to say about how they came to their jobs. Private school principals have quite a bit more teaching experience than their public counterparts —the gap is almost four years for principals in Catholic schools, and over five years for those in the elites and "other privates."[11] This is consistent with the hierarchic organization of public sector education: its career ladder offers teachers early opportunities for moving into a host of subordinate administrative positions (such as assistant principalships), followed by subsequent opportunities for moving up in status and salary. As this implies, principals also come to their jobs with different motivations. Private principals are more likely to stress "control over school policies," while public principals place greater emphasis on "preference for administrative responsibilities," a "desire to further [their] career[s]," and an interest in advancing "to a higher administrative post."

The typical career orientations of principals in the two sectors thus appear to be quite different. Public principals tend to disembark from teaching relatively early, get on an administrative track, and take the job of principal to keep the train rolling. Private principals are scarcely on a track at all. They stay in teaching longer, and their view of the principalship focuses more on its relation to the school than on its relation to their movement up the educational hierarchy.

How the principal performs on the job is a function of many things, not just the values and experiences noted here. They would, however, appear to have a direct bearing on one aspect of performance consistently singled out in the effective schools literature: instructional leadership. Teachers in the ATS study were asked questions about the quality of the assistance they received in regard to instructional problems, and their responses indicate strong differences across the sectors. As judged by their own teachers, private principals are more effective in this important area of leadership than public principals are. Again, this may be due to a variety of factors. But the simple fact that the public principal has far less teaching experience (which itself has roots in his or her distinctive career orientation) is in itself likely to affect rapport with teachers, self-perception of instructional role, and other aspects of the job as they pertain to teaching. It is not surprising to find that instructional leadership is more effective in private schools.

Finally, the ATS teachers were asked to evaluate their principal with regard to a range of leadership-related qualities bearing on knowledge of school problems, communication with the staff, clarity and strength of purpose, and willingness to innovate. Constructing a general index of leadership from these items, we find that by these criteria teachers rate private principals to be better all-around leaders than public principals. This result is more likely to reflect the operation of general environmental conditions than the earlier one on instructional leadership. Principals in the public sector are forced to operate in much more complex, conflictual circumstances in which educational success is more difficult to achieve regardless of the principal's (perhaps considerable) abilities and qualifications. If anything, however, it is plausible to suggest that the public principal's lack of teaching experience and a hierarchic career orientation probably contribute to these leadership problems.

While these findings only begin to scratch the surface, it does appear that public and private school principals are quite different in important respects. They have different backgrounds, different career orientations, and—whatever the true constraints on their performance might be—they are evaluated differently by their teachers: principals in private schools are more highly regarded as leaders.

The Organization: Goals and Policies

Given what we know of their environments, there is every reason to expect that public and private schools should adopt very different orientations toward the education of their students. Because public schools must take whoever walks in the door, they do not have the luxury of being able to select the kind of students best suited to organizational goals and structure—it is the latter that must do virtually all the adapting if a harmonious fit is to be achieved. In practice, this means that the pursuit of educational excellence must compete with much more basic needs—for literacy, for remedial training,

Table 7. School Structure

Characteristics	Catholic	Other Private	Elite Private
Goals			
Basic literacy	-1.59 (8.83)	-.83 (5.81)	-1.10 (.59)
Citizenship	-1.12 (6.06)	-1.04 (7.11)	-.96 (.50)
Good work habits	-.92 (4.68)	-.52 (3.37)	-.26 (.13)
Occupational skills	-.89 (4.60)	-.77 (5.01)	-.98 (.49)
Academic excellence	.10 (.48)	.41 (2.57)	.94 (.45)
Personal growth	.47 (2.33)	.12 (.78)	.69 (.33)
Human relations	.24 (1.19)	.11 (.71)	.34 (.16)
General graduation requirements			
English and history	.61 (3.09)	.51 (3.26)	.57 (.28)
Science and mathematics	.34 (1.73)	.88 (5.77)	1.78 (.88)
Foreign language	1.28 (7.88)	1.61 (12.47)	3.33 (1.96)
School-wide homework policy	.13 (.65)	.48 (3.06)	.90 (.44)
Goal clarity	.64 (3.32)	.80 (5.24)	.80 (.40)
Goal disagreement	-.35 (1.80)	-.85 (5.59)	-.55 (.27)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

for more slowly paced instruction. In addition, there is the hierarchic structure of democratic control to ensure that a range of actors and diverse, often-conflicting interests are brought to bear in decisions about what the public school ought to be pursuing and how. As in other areas of politics, the thrust is toward compromises and "solutions" (see also Powell, Farrar, and Cohen 1985) that reflect the lowest common denominator—and often a great deal of ambiguity and internal inconsistency as well. This is to be expected when an important function of the decision-making process is conflict resolution. The process is unavoidably a political exercise, not an analytical attempt at problem solving.

Private schools are largely unconstrained in comparison, both in the selection of students and in the determination of organizational goals. It is only reasonable to suggest that a given private school is likely to have clearer and more homogeneous goals than a given public school. Aggregate comparisons, however, are more uncertain because the private sector is comprised of so many different types of schools; an elite school will emphasize academic excellence, but a religious or military school may have quite different priorities—although we would still expect them to be relatively clear and homogeneous compared to the publics.

Despite such uncertainties, the comparisons across sectors are quite uniform (see Table 7). In terms of general goals, public schools place significantly greater emphasis on basic literacy, citizenship, good work habits, and specific occupational skills, while private schools—regardless of type—are more oriented by academic excellence, and personal growth. For the most part, these sorts of differenes are what we should expect in view of the more fundamental differences in student bodies and governing structures. Most obviously, public schools would ordinarily find it politically and organizationally very difficult to place high priority on academic excellence.

Whether these goals become reflected in school structure and performance depends on whether they are upheld by specific policies and clearly discerned by the staff. As Table 7 suggests, there are definite differences across sectors in these respects. To begin with, the private schools have more stringent minimum graduation requirements; their students, regardless of track, must take significantly more English and history, science and math, and foreign language than must public students in order to graduate. In science, math, and foreign language the differences range up to two years.

Private schools also have stricter homework policies. This is particularly true of the elites and "other privates," most of which establish schoolwide daily minimums per subject, strongly encourage homework, or, in cases where faculty are overzealous, set daily maximums per subject. In contrast, 90% of all public schools leave the amount of homework entirely up to teachers. Catholic schools fall in between these extremes.

These differences in goals and policies are accompanied by differences in their clarity and their acceptance by organization members—key factors in their translation into organizational action. Private teachers uniformly report school goals as clearer—and more clearly communicated by the principal—than public teachers report. In addition, there is less disagreement among the school priorities reported by teachers in private schools. In general, private schools tend to possess a clarity and homogeneity of educational purpose that does set them apart from public schools, at least on average. They place more emphasis on academic excellence, have stricter graduation requirements, and have tougher homework policies. And their staff members have clearer, more consistent conceptions of what their organizations are supposed to be achieving. These are, of course, stereotypical characteristics of "effective shools." They are also characteristics that, due to the differential operation of politics and markets, would seem extremely difficult for public schools to develop in the same degree.

The Organization: People, Decisions, Operation

What should public and private schools look like on the inside? What might we expect in general about their structures, processes, and personnel? A widely accepted notion in organization theory is that environmental complexity is reflected in organizational complexity (Lawrence and Lorsch 1967). For rather obvious reasons, then, public schools should prove far more complex than their private counterparts; and existing studies indicate that this is actually the case (Scott and Meyer 1984). Moreover, their very complexity of structure and heterogeneity of goals suggest that public schools may often be "loosely coupled," characterized by relatively autonomous centers of activity and decision making. Private schools, on the other hand, would seem to approximate classical notions of organization. They have simpler, more stable, less threatening environments, and goals that are fewer in number, clearer, and more narrowly based—characteristics that facilitate the centralized direction of goal pursuit. This would seem to be consistent with the private schools' reputation for rigid curricula, traditional instructional methods, and strong principals.

A politics-and-markets perspective cannot hope to tell us everything we might want to know about organizational structure and process, but it does tend to point us in a different direction. The critical fact about the public school environment is not just that it is complex but that it literally imposes decisions about policy, structure, personnel, and procedure on the school. Thus, while the school may well adapt to environmental complexity by developing an internal complexity of its own, its range of choice is severely constrained—for a great many potential adaptive adjustments are simply ruled out by environmental fiat. Conversely, the private school is not only blessed with a relatively simple environment but with a much broader range of organizational options in adapting to it.

Consider, in particular, the most crucial agent of organizational performance: the teacher. As we have seen, the public school principal is far less able than the private school principal to staff the organization according to his or her best judgment. The public principal may value expertise, enthusiasm, collegiality, communication skills, creativity, or any number of qualifications related to the school's goals but simply has less power to obtain teachers who possess them or eliminate ones who do not. This should tend to promote staff heterogeneity and conflict. Teachers may reject the principal's leadership, dissent from school goals and policies, get along poorly with their colleagues, or fail to perform acceptably in the classroom—but the principal must somehow learn to live with them. When these teachers are represented by unions, as they normally are, leadership difficulties are magnified and an important wedge is driven between the principal and the staff, a wedge that promotes formalized decision procedures, struggles for power, and jealousies over turf. "Professionalism" takes on new meaning—as a justification for placing decision power in the hands of teachers rather than the principal.

Private schools are not immune from personnel problems and struggles for power. But the principal, having much greater control over hiring and firing, can take steps to recruit the kinds of teachers he or she wants and weed out the rest. It also means that teachers have a strong inducement to live up to the principal's criteria on a continuing basis. By comparison to the public school counterpart, then, the private school principal is in a position to create a "team" of teachers whose values, skills, and willingness to work together tend to mirror those qualifications the principal deems conducive to the pursuit of organizational goals. At the same time, the principal is in a position to make teacher professionalism work for, rather than against, him or her. Without real threat to his or her own authority or control, the principal can encourage teacher participation in decision making, extend teachers substantial autonomy within their own spheres of expertise, and promote a context of interaction, exchange of ideas, and mutual respect.

The data from the ATS study seem to provide strong support for this general line of reasoning. As outlined in Table 8, principals and teachers simply have higher opinions of one another in the private sector. Private principals consistently claim that a larger percentage of their schools' teachers are "excellent," suggesting that they are more confident in the abilities of their own staff members than public school principals are. Private sector teachers, in turn, have better relationships with their principals. They are consistently more likely to regard the latter as encouraging, supportive, and reinforcing; and, as we saw earlier, they have higher regard for their principals as effective organizational leaders.

Private school teachers also feel more involved and efficacious in important areas of school decision making that bear on their teaching. In particular, they feel more influential over schoolwide policies governing the curriculum, student behavior, teacher in-service programs, and the grouping of students of differing abilities. Regarding issues of special relevance to the classroom, they believe they have more control over text selection, course content, teaching techniques, disciplining students, and, in the Catholic schools, determining the amount of homework to be assigned. (The non-Catholic private teachers feel constrained by the schoolwide homework policies identified earlier.) Even on matters of hiring and firing,

Table 8. Staff Relations

Characteristics	Catholic	Other Private	Elite Private
Percent excellent teachers	.40 (2.07)	.78 (5.05)	1.16 (.58)
Principal-teacher relations	.44 (2.29)	.90 (5.93)	.96 (.48)
Teacher influence and control			
Student behavior codes	.95 (5.15)	1.00 (6.84)	1.04 (.54)
In-service programs	.32 (1.64)	.77 (5.05)	.41 (.20)
Ability groupings	1.24 (7.20)	1.32 (9.69)	1.37 (.76)
Curriculum	1.01 (5.66)	1.22 (8.59)	1.22 (.66)
Text selection	.66 (3.46)	.78 (5.11)	.74 (.37)
Topics taught	.50 (2.62)	.94 (6.23)	.53 (.27)
Techniques	.70 (3.58)	.40 (2.61)	1.06 (.52)
Discipline	1.34 (7.17)	.46 (3.12)	.83 (.42)
Homework	.63 (3.19)	.33 (2.14)	-1.16 (.57)
Hiring	.54 (2.63)	.38 (2.38)	.93 (.44)
Firing	.55 (2.75)	.16 (1.04)	.21 (.10)
Teacher-teacher relations			
Curriculum coordination	.60 (3.11)	.67 (4.37)	.96 (.48)
Teaching improvement	.60 (3.21)	1.06 (7.21)	1.03 (.53)
Collegiality	.90 (5.34)	1.58 (11.82)	.82 (.47)
Success not beyond personal control	.79 (4.19)	.54 (3.59)	1.23 (.62)
Doing best not waste of time	1.12 (5.39)	.96 (5.85)	1.78 (.82)
Job satisfaction	.57 (3.37)	.54 (4.06)	1.01 (.58)
Teacher absenteeism	.58 (3.00)	.73 (4.74)	.49 (.24)
Lowest teacher salary	-.75 (3.92)	-.86 (5.72)	.39 (.19)
Highest teacher salary	-.76 (3.87)	-.57 (3.65)	.91 (.45)

Note: Reports regression coefficients and t-scores (in parentheses) for dummy variable regression models in which the dependent variable is standardized.

private teachers believe they are more influential—this, despite the almost complete absence of unions in their sector.

Relative harmony between private principals and private teachers is matched by relative harmony among the private teachers themselves. On a personal level, relationships are more collegial in the private sector. Stated in the plain terms of the survey, private teachers are more likely to believe that they "can count on most staff members to help out anywhere, anytime—even though it may not be part of their official assignment" and ultimately that "the school seems like a big family." On a professional level, private teachers give greater evidence of mutual involvement and support. They are more likely to know what their colleagues are teaching, to coordinate the content of their courses, and to observe one another's classes. They also spend more time meeting together for the purpose of discussing curriculum and students.

It is no surprise, then, that private school teachers also feel more efficacious than public school teachers. Unlike their public counterparts, they do not believe their success is beyond their control, and they do not feel it is a waste of time to do their best. Overall, private school teachers are much more satisfied with their jobs. It is no wonder, then, that private school teachers have better attendance records nor that they tend to work for less money. Private school teachers are trading economic compensation and formal job security for superior working conditions, professional autonomy, and personal fulfillment. Public school teachers are doing precisely the opposite.

In short, private schools do tend to look more like "teams." By their own account, teachers have better relationships with principals, are more integrally involved in decision making, interact more frequently and productively with their colleagues, and feel more positively about their jobs and their organization. According to their principals, they are higher-quality teachers as well. As professionals, it appears they are given much greater reign in a private setting, which gives them opportunities to put their ideas and skills to use through a level of active involvement and a sharing of power that teachers in the public sector generally cannot expect. The key to explanation is anchored in a more fundamental feature of the sectors: private leaders have the freedom to chose their own professionals, public leaders do not.

Conclusion

The Administrator and Teacher Survey provides the first opportunity to document public-private differences by means of a large, representative sample of schools, and its findings dovetail nicely with major lines of argument in the education literature. If it is true, as Coleman, Hoffer, and Kilgore (1982) and Coleman and Hoffer (1987) have claimed, that private schools outperform public schools on academic grounds, and if the effective schools research is basically correct in the characteristics it tends to associate with effectiveness, then we should find that private schools disproportionately possess these characteristics.

That is just what we find. Private schools have simpler, less constraining environments of administrators, school boards, and parents. They are more autonomous and strongly led. They have clearer goals and stricter requirements, and they put greater stress on academic excellence. Relations between principals and teachers and among teachers themselves are more harmonious, interactive, and focused on teaching. Teachers are more involved in policy decisions, have greater control over their work, and are more satisfied with their jobs.

We have tried to do more here than present findings and relate them to existing work, however. We have tried to develop an institutional perspective that suggests why schools should be expected to differ across the sectors. This perspective arises from our belief that institutional context has pervasive consequences for the organization and operation of all schools, consequences more far-reaching than most of the literature tends to suggest (Pfeffer and Salancik 1979).

Public schools are products of our democratic institutions. They are subordinates in a hierarchic system of control in which diverse constituency groups and public officials impose policies on local schools. It is no accident that public schools are lacking in autonomy, that principals have difficulty leading, and that school goals are heterogeneous, unclear, and undemanding. Nor is it an accident that weak principals and tenured, unionized teachers struggle for power. These sorts of characteristics constitute an organizational syndrome whose roots are deeply anchored in democratic control as we have come to know it.

Private schools are controlled by society too, but not through politics or bureaucracy. They make their own decisions about policy, organization, and personnel subject to market forces that signal how they can best pursue their own interests. Given their substantial autonomy—and given the incentives for autonomy that are built into the system—it is not surprising to find that principals are stronger leaders; that principals have greater control over hiring and firing; that principals and the teachers they choose have greater respect for, and interaction with, one another; and that teachers—without conflict or formal requirement—are more integrally involved in policymaking. These sorts of characteristics are bound up with one another, and they jointly arise from the institutional environment. Different institutions promote different organizational syndromes.

If this is essentially correct, the standard proposals for reforming public schools are misconceived. It is easy to say, for instance, that schools should have greater autonomy or that principals should be stronger leaders. But these sorts of reforms are incompatible with the "one best system" and cannot succeed. Politicians and bureaucrats have little incentive to move forcefully in these directions. Their careers are tied to their own control over the schools, and they are unavoidably responsive to well-organized interests that have stakes in the system's capacity to impose higher-order values on the local schools. Restricting autonomy is what democratic control is all about.

It is also about power, about who gets to have how much say in the control of schools. Reformist notions that the various actors should work together in the best interest of the schools are doomed by the institutions of democratic control, which guarantee conflict of interest, struggle for advantage, and resort to formally enforced "cooperation." Reforms calling for even the simplest changes—testing veteran teachers for minimum competence, say—will normally fail if they threaten established interests. Their bearing on school effectiveness has little to do with their political feasibility.

Reformers must reckon with the possibility that the measures they support, particularly those arising from the effective schools research, are often inconsistent with our current framework of democratic control. The public schools cannot be anything we might want them to be. They must take organizational forms compatible with their surrounding institutional environments. It may well be, then, that the key to school improvement is not school reform, but institutional reform—a shift away from direct democratic control.

This does not mean that the public schools must be freed from all democratic governance. But it is instructive that the private schools, which are products of an institutional system that decentralizes power to the producers and immediate consumers of educational services, tend to develop precisely the sorts of organizational characteristics reformers want the public schools to have. Some sort of voucher system, combining broad democratic guidance with a radical decentralization of resources and choice, is at least a reasonable alternative to direct control—one that might transform the public schools into different, more effective organizations, while still leaving them truly public.

Even if this or some other alternative is someday shown to have compelling features, however, democracy probably cannot get us from here to there. Any proposal to shift away from prevailing institutions is so threatening to established interests that it stands little chance of political victory. Because a shift in institutional control may be the one reform that makes all the others possible, the uncomfortable reality may simply be that all the others are not possible.

Notes

An earlier version of this paper was presented at the 1987 annual meeting of the American Political Science Association, Chicago. The first draft appeared as the Brookings Institution's Governmental Studies Discussion Paper No. 1.

1. The responsiveness of residential decisions to differences in the quality of public services, includ-

ing education, has been extensively investigated and generally found to be quite imperfect (Rose-Ackerman 1983).

2. This is not to say that intelligent guidance from the center—e.g., about important innovations in curriculum and methods—is unimportant, only that education appears to benefit from a balance of control and autonomy favoring the teachers and the school (e.g., Carnegie Forum on Education and the Economy 1986).

3. On the logic of control and its implications for bureaucratic forms of organization, see, e.g., Williamson 1975. For a review with applications to politics, see Moe 1984. On the historical development of the educational bureaucracy, see Tyack 1974 and Peterson 1985.

4. There are some interesting complications that we cannot dwell on here without getting too far afield. Mayors, for instance, sometimes attempt to avoid the risks and pressures of educational politics by minimizing their own roles and shifting authority to others (e.g., members of the school board). More generally, politicians sometimes find it advantageous to create agencies that are insulated in some measure from political influence, including their own influence. These maneuvers, however, never put an end to politics or political control—and the maneuvers themselves are reversible through subsequent political maneuvers. Through it all, the authority to impose higher-order values is still there, and those with access to it still have incentives to use it to get what they want.

5. This study has been pummeled from all angles. See, e.g., Bryk 1981; Goldberger and Cain 1982; Guthrie and Zusman 1981; Heyns and Hilton 1982; Murnane 1981. It seems to us, however, that the thrust of the Coleman-Hoffer-Kilgore argument has largely withstood these attacks. In more recent research by Coleman and Hoffer (1987) it has also been strongly reinforced.

6. When the ATS data are merged with those from HSB and its follow-ups (the details of which are available from the National Center for Education Statistics), they offer a unique foundation for exploring the connections among environment, organization, and student achievement. That is the purpose of our larger project, of which this paper is a part.

7. Studies of organization, as opposed to student achievement, have dealt almost entirely with public schools. Most of what is known about the organization of private schools is derived from case studies or studies of limited samples (Cibulka, O'Brien, and Zewe 1982; Erickson 1982; Greeley 1966; Peshkin 1986; Sanders 1981). Systematic national surveys are rare (Abramowitz and Stackhouse 1980; Kraushaar 1972).

8. The procedures employed in constructing indexes and measures of the variables are detailed in an appendix available from the authors.

9. Throughout this paper coefficients will be called "statistically significant," or simply "significant," if they satisfy a two-tailed t-test at a probability level of .05. The results for the elite schools will not, however, be evaluated in this fashion. The elite schools are not a sample, but a population—the schools with the most National Merit semi-finalists in 1978. As such, it is arguably inappropriate to make statistical inferences from the "sample" to the population. In any case, the number of elite schools is too small, especially after weighting, to produce t-scores in the necessary range of 2.0.

10. Private schools may also offer merit pay; however, only the "other private" sector makes significantly greater use of it. Catholic schools do not differ from public schools in providing merit pay.

11. Recall that the coefficients reported in the tables are based on standardized measures of the dependent variables. When converted back to their original metric, the coefficients for teaching experience are equivalent to the years reported in the text.

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THE POLITICAL ECONOMY OF STATE MEDICAID POLICY

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We develop and test a market-based model to explain variations in states' welfare medicine policy decisions. The empirical results support the model of state policy outputs, indicating that states' spending efforts for welfare medicine are most sensitive to the supply of services within their borders. We learn in addition that spending effort declines with demand for services, indicating that the states spending the highest proportions of total personal income for the program are those who need it most and can afford it least. Measures of political system development affect spending effort positively and significantly, suggesting that ideology, diversity of interests, and administrative professionalism increase states' welfare efforts.

We construct a supply and demand model of state policymaking and test its applicability to welfare medicine policies in the states. Through this exercise we seek to contribute to the growing body of empirical political economy research, which incorporates economic and political theories in models of policy and politics. As stated elsewhere (see, e.g., Chubb 1985; Plotnick and Winters 1985), neither wholly political nor wholly economic explanations are adequate for some policy outputs. Better explanations are gained by blending elements of each approach.

Policy-making environments may be likened to markets in which demand and supply influence policy provision. Political mechanisms are designed to link citizen demands for government services to policies. Demand is expressed most directly when citizens or groups convey preferences about governmental actions to policymakers. Supply in policy markets is set by the development of service delivery systems, ability to pay, and by normative and technical constraints.

Embodied in the neoclassical theory of markets is the assumption that consumers share information equally; that assumption does not hold in policy markets (see Moe 1984). Some citizens and groups are better equipped than others to monitor or participate actively in the governmental process and express their demands more clearly than others. Hence there is an information problem, creating an asymmetrical market.

As issues become more complex and abstract, the size of the attentive public decreases and the role of experts from the private and public sectors increases. The public's ability to express demands clearly is hampered by its poor and at times poorly targeted attention to issues. Issues enter the agenda and receive some public attention, government may or may not act, issues' perceived importance may fade, and the public's attention shifts to other concerns (Downs 1972). The lack of widespread public attention to policy problems prejudices the policy market toward organized interests that are better equipped than the disorganized public to effect change. Even where elective officials and bureaucrats work to protect the public's interests, broadly conceived, organization is crucial in determining who gets what from government (Olson 1982, 1985).

The supply of policy outputs is determined in part by wealth. Public spending generally increases with the wealth of jurisdictions (Aronson and Hilley 1986, 1–10), leading some writers to conclude that policy possibilities are determined by economics (Dye 1966; Lewis-Beck 1977). However, group conflict and general political development also increase with wealth (Olson 1982). Consequently, political and economic development are complementary (Godwin and Shepard 1976; Hanson 1984; Plotnick and Winters 1985). As the potential for budgetary expansion grows, demand for governmental action increases. Supply of and demand for policy are aligned closely.

The states' welfare medicine policy decisions nicely illustrate policy market arrangements. A state's medical care policies are determined partly by countervailing powers of providers (supply) and recipients (demand). In turn, policy may affect supply and demand. Characteristics of political, social, and economic environments define the bounds within which decisions may be made and constrain policy outputs. A model of policy determination may be expressed in terms of

$$\text{demand} = f\,(\text{supply, policy, constraints}) \quad (1)$$

$$\text{supply} = f\,(\text{demand, policy, constraints}) \quad (2)$$

$$\text{policy} = f\,(\text{supply, demand, constraints}) \quad (3)$$

Individual politicians and legislatures are biased toward spending (Downs 1957; Hayes 1981; McCormick and Tollison 1981). Wealth, political participation, and political competition constrain the scope of policy and mediate legislative spending decisions. Supply, demand, and policy are interrelated, each simultaneously influencing the shape of the others.

We seek to make three major points through this exercise. First, we specify and test a model of policy determination. Second, we demonstrate the mutual influence of the environment on policy and of policy on the environment, which cannot be determined properly absent the use of simultaneous estimation techniques. Third, by measuring state policy in a manner that assesses the relative effort they devote to Medicaid spending, we cast state policy differences in a more revealing light than that cast by liberal-conservative or wealthy-poor distinctions. Analysis of the determinants of the real financial costs of a policy borne by a state's citizens calls for construction of a model more complex than that presented in the academic literature on policy determination.

Following this introduction we describe Medicaid, the policy to be analyzed. In the third and fourth sections, respectively, we develop the theoretical and measurement models and present results of the empirical tests. The fifth section consists of a discussion of our empirical findings in light of the theoretical model. Conclusions are drawn in the final section.

A Medicaid Primer

Medicaid, a state-federal program, is the primary vehicle for providing the poor or near-poor access to health services in the states. It is a means-tested social insurance program in which the state acts as insuror of last resort, paying both private and public sector medical vendors directly for services delivered to beneficiaries. It is paid for by the states and the national government. The national government share, which ranges from 50% to 77% of total state spending, is determined by a formula based on states' per capita incomes.

States administer Medicaid within federal guidelines. Receipt of federal funds is contingent upon state compliance with federal standards regulating the type and amount of services paid for under state programs, states' eligibility determination policies, and state provider payment policies. State-federal relations under Medicaid are typically strained; each wishes to control the program to the greatest extent possible while minimizing its financial burden.

Medicaid is expensive. Total state-federal spending for service delivery was $40.7 billion in 1986. Medicaid spending, like that for health care generally, is often characterized as hard to control (Grannemann and Pauly 1983; Holahan and Cohen 1986). The program was created in 1966 with the stated goal of ensuring the indigent populations' access to mainstream medical services. Because responsibility for public welfare programs traditionally fell to the states, they took the lead in developing programs and sought to guarantee access, showing little concern for program costs. Congress had little information about potential costs when legislation forming the program was passed, and spending was higher than early projections (Thompson 1981). During the late 1970s the national government sought to reduce spending and shifted its emphasis from guaranteeing access to controlling cost.

The Carter administration's most visible contribution to Medicaid reform lay in the creation of the Health Care Financing Administration (HCFA), lodged in the Department of Health and Human Services, in 1977. The major task of HCFA is to administer Medicare and Medicaid. Its presence has strengthened the national government's ability to oversee state Medicaid bureaucracies. Congress passed legislation aimed at reducing Medicaid spending during the early 1980s. The major vehicle for effecting changes in Medicaid financing arrangements was the Omnibus Budget Reconciliation Act of 1981 (OBRA 1981), in which the national government reduced its rates of financial participation and increased states' flexibility to design reimbursement practices and to pursue other administrative reforms. Cost containment efforts initiated as a result of the act initially slowed the rate of spending increases dramatically. Efforts to reduce expenditures continue (Holahan and Cohen 1986, 13–16).

State administrators, responding to their own budgetary constraints as well as to changes wrought by OBRA 1981, began to seek efficiencies and savings more vigorously than before. Since total expenditures are a product of eligibility, utilization, and the cost of services, the obvious expenditure control strategies were to hold each component as low as possible (Davidson 1980; Hanson 1984). Thus the 1980s have been a period of tinkering and experimentation with Medicaid programs, some of which has reduced rates of spending increases (Holahan and Cohen 1986; Cromwell, Hurdle, and Schurman 1987; Miller n.d.). Despite lessened cost increases during this decade, spending remains high and consumes an increasing portion of state budgets, leading state and federal administrators to seek further cost control measures.

Controlling Medicaid spending is difficult. Some of the Medicaid spending problem is a function of program design: states set eligibility criteria and other program characteristics and wait to see how much it costs in a given year. If, for example, a state's economy falters and unemployment rises, the number of Medicaid eligibles increases, leading to greater provider billings for which the state is legally liable. Thus total expenditures are often beyond legislative control. Consequently, the program cannot be budgeted in a traditional sense and represents a wild card in state and national budgets.

Part of the cost control problem is due to the nature of health care as a consumer good. The price elasticity of demand for a commodity, defined as the percentage change in quantity demanded resulting from a 1% change in price, provides guidance for modeling transactions. Health care services' price elasticity of demand is low. Consumers of health care purchase services under conditions of uncertainty and expect providers to deliver appropriate treatments (Arrow 1963). Providers are regulated by personal and professional norms; their expertise and ethics are thus highly valued and important for the continued functioning of the health care system. Health care markets, given providers' control of expertise and information, are asymmetrical, thereby violating one of the central assumptions of neoclassical economics. Medicaid eligibles in the vast majority of states pay nothing out-of-pocket for services received. Hence service recipients do not associate a monetary price with the receipt of Medicaid-paid health care. Vendors, realizing that charges do not deter eligibles' service utilization, may inflate their billings by delivering unnecessary services (Auster and Gordon 1974). Even where providers do not seek to increase billings, the threat of malpractice suits, abetted by the low price elasticity of demand, induces them to practice medicine defensively, which increases billings.

The major issue revolving around Medicaid during its formative years was that of service delivery. It was created to ensure eligibles' access to health care. Access has been attained (Long, Settle, and Stuart 1986) but at a high financial cost. The expenditure control issue is now the most pressing concern for states and the national government.

The Political Economy of Medicaid

Medicaid is unique among public welfare programs because its functioning depends on the cooperation and support of private practitioners. As a result, modeling Medicaid requires consideration of the peculiar economics of health care, which are confounded further when viewed within the confines of a policy market. We do not know how much medical care is demanded by an informed consumer. Most health care recipients have little understanding of what constitutes appropriate care and, especially where the only expense is time (and maybe a little indignity), are susceptible to receiving inappropriate treatments by sometimes unscrupulous, but usually just cautious, providers. Demand for care leads to its being supplied. Supply begets demand. Supply and demand are best estimated simultaneously.

Medicaid programs cannot function without the cooperation of providers, and efforts to regulate various sectors of the health care industry have proven difficult. Medicaid decisions must be framed in a manner in which they are acceptable to the provider community (Davidson 1982). Policymakers' perceptions of Medicaid and subsequent program design decisions are colored by their estimates of its costs to the state; cost consciousness influences other aspects of the program. Policy decisions concerning eligibility affect demand directly. Likewise, expansion of eligibility may increase the number of physicians willing to participate in Medicaid by broadening the program's recipient pool and thus removing some of the stigma associated with being a Medicaid recipient or a Medicaid provider. Policy, supply, and demand for services determine one another; they also share relationships with some political and socioeconomic constraints.

In this research we model variations in

state Medicaid spending effort, demand for Medicaid services, and potential supply of services in a system of three simultaneous equations. Measures of supply, demand, and policy are endogenous to the system; measures of states' social, economic, and political characteristics are exogenous.

Measuring Supply, Demand, and Policy

The proportion of total state personal income spent on Medicaid vendor payments (minus federal financial contribution) during 1982 is used as a proxy for welfare effort. Expenditure-based policy indicators are open to criticism on both conceptual and empirical grounds. Hanson (1984, 313–15) provides a convincing conceptual criticism, arguing that aggregate or per capita spending measures do not indicate policy decisions, per se, leading to misrepresentation of the results of existing models. He demonstrates that measures of the scope of state policies better indicate their distributional properties. Empirical criticisms are related to conceptual problems that arise when using aggregate or per capita spending measures: their use lends itself to income-driven explanations for differences in spending (see, e.g., Aronson and Hilley 1986, 31–47).

The spending-based measure used herein, however, captures states' welfare efforts and is not necessarily income-driven. It is empirically distinct from aggregate and per capita spending measures.[1] We measure the relative amount of resources policymakers devote to providing health insurance to their indigent populations. The range of behaviors is wide. Some poor states' welfare efforts are disproportionate to the adequacy of their welfare grants package. Some wealthier states exert lower effort yet provide more generously than poorer states. Still other states spend a moderate amount of their revenues on welfare and provide fairly adequate grants (Albritton 1983, 392–93).

Supply and demand are difficult to measure accurately. Empirical studies show that utilization of services increases with the number of providers in an area (Fuchs 1978; Roemer 1961), especially when the recipient of services pays little out of pocket (Wilensky and Rossiter 1983). The total number of registered physicians in nonfederal employment in each state during 1982 is used as a proxy for supply in this research. Use of the physician measure reflects their portrayal as agents who channel patients to other services (e.g., hospital inpatient care, etc.) through referrals (Pauly and Redisch 1973).[2]

We use a simple measure of demand: the unduplicated count of the number of persons receiving Medicaid services of any kind during 1982. Two other indicators, poverty population and eligibility pool, were considered but discarded. Poverty population is inappropriate since many of the poor are not eligible for Medicaid because of differences in state eligibility policies. The use of an estimate of potential eligibility in a given state as a measure of demand was discarded because persons eligible for Medicaid who fail to enroll do not reflect demand for the program. In the final analysis recipiency (i.e., realized demand) most closely approximates the construct we wish to make operational.

Operational Models

We embrace the premise that socioeconomic and political characteristics of states are complementary, not competitive, in their influence on policy outcomes (Godwin and Shepard 1976; Hanson 1983, 1984; Plotnick and Winters 1985). States' socioeconomic and political traits may facilitate or constrain Medicaid effort. Wealth exerts a positive influence on the breadth of welfare coverage (Albritton 1983; Hanson 1983; Plotnick and Winters 1985). Similarly, higher wealth is associated with more expansive Medicaid policy coverage (Davidson 1980; Hanson 1984). State policies reflect decisionmakers' collective understandings of how best to use available resources. Available resources limit the range of policy possibilities.

Political and economic development are crucial determinants of governmental actions. First, increased development generally is associated with increased wealth. Second, political development occurs alongside social and economic development (Sharkansky and Hofferbert 1969; Sullivan 1973). As social and economic development advance, interests become more diverse. Where interests are diverse there is increased political conflict and greater demand for government action. Development increases demand for distributive decisions.

Institutions adapt to changes in the political environment; complex institutions and institutional relationships are needed to process the demands of diverse publics. Well-developed political systems characteristically have competitive political parties, highly diverse interest groups, professionalized bureaucracies and legislatures, and high voter turnout and citizen participation. We seek to portray variations in the endogenous variables of interest as functions of varying levels of economic and political development in the states.

Demand for, and supply of, medical care are best expressed as functions of one another, an endogenous policy measure, and exogenous measures of states' economic and demographic characteristics. Policy is a function of endogenous measures of demand for, and supply of, care and of exogenous measures of states' economic, demographic, and political characteristics. Equations operationalized below follow the forms specified in equations 1–3. Demand for Medicaid policy has no explicitly political content and takes the form

$$\begin{aligned}\text{DEMAND} = {} & a + b_1\#\text{SUPPLY} \\ & - b_2\text{PRICE} + b_3\text{URBAN} \\ & + b_4\text{UNEMPLOYMENT} \\ & + b_5\#\text{MEDICAID EFFORT}, \qquad (4)\end{aligned}$$

where DEMAND = unduplicated count of persons receiving Medicaid services, #SUPPLY = estimated number of nonfederal physicians, PRICE = expenses per inpatient hospital day, URBAN = urban population, and #MEDICAID EFFORT = estimate of the proportion of total personal income devoted to Medicaid.

As expressed in equation 4, we expect demand for Medicaid services to be driven by supply, policy, economic, and demographic features of state environments. We expect demand to rise with increases in the supply of services available, urbanization, unemployment, and Medicaid effort. Demand is expected to diminish in the face of higher prices.

Since no out-of-pocket expenses are incurred by recipients of Medicaid services, charges are not generally thought to deter eligibles' seeking care. However, Medicaid reimbursement rates, set by the state, come into play. Setting Medicaid reimbursement rates low relative to private reimbursement rates or to the actual cost of providing care diminishes providers' incentives to participate in the program (Davidson 1982; Grannemann and Pauly 1983; Long, Settle, and Stuart 1986). Medicaid recipients, for whom the psychological discomfort of seeking charity services already depresses service utilization (Dutton 1978), may be even less prone to seek care in the face of provider dismay with low reimbursement rates. Ergo, low reimbursement relative to prices may suppress both supply and demand of Medicaid.

Supply of Medicaid services is explained somewhat as demand is. The supply of medical care services generally increases along with economic development and urbanization. The profitability of medical enterprises depends on characteristics of the market for care. Hospitals require an adequate market to operate in the black. Physicians prefer to practice where there are sufficient numbers of persons to constitute an adequate caseload, other physicians, and adequate research and clinical facilities (O'Donoghue 1976). Following this logic, supply is expressed

$$\text{SUPPLY} = a + b_6\#\text{DEMAND} + b_7\text{PRICE} + b_8\#\text{MEDICAID EFFORT} + b_9\text{URBAN} + b_{10}\text{INCOME}, \quad (5)$$

where SUPPLY = number of nonfederal physicians, #DEMAND = estimate of the unduplicated count of Medicaid service recipients, PRICE = average expenses per inpatient day, #MEDICAID EFFORT = estimate of the proportion of state's total personal income devoted to Medicaid, URBAN = urban population, INCOME = personal income.

The supply equation 5 reflects the notion that providers locate in strong markets. We expect the endogenous measures of demand (#DEMAND) and state Medicaid effort (#MEDICAID EFFORT) to influence supply positively. Physicians choose to locate in strong markets; the absolute number of persons eligible for Medicaid increases with income and urbanization. Both urbanization and income influence the supply of health providers directly and positively. The positive influence of price on supply is consistent with the logic that providers locate where they can maximize income.

Estimates of demand and supply reflect the developmental model discussed above. In equation 6 we model variations in states' Medicaid spending efforts. Equation 6 differs from equations 4 and 5 inasmuch as we incorporate measures of political development in our explanation. Medicaid effort is estimated

$$\text{MEDICAID EFFORT} = a - b_{11}\#\text{SUPPLY} - b_{12}\#\text{DEMAND} + b_{13}\text{PRICE} + b_{14}\text{IDEOLOGY} + b_{15}\text{GROUPS} + b_{16}\text{BUREAUCRACY} + b_{17}\text{URBAN}, \quad (6)$$

where MEDICAID EFFORT = the proportion of total personal income devoted to Medicaid spending, #SUPPLY = estimate of the number of nonfederal physicians, #DEMAND = estimate of the unduplicated count of Medicaid service recipients, PRICE = average expenses per inpatient day, IDEOLOGY = Americans for Democratic Action rating of states' congressional delegation, GROUPS = Morehouse (1981) index of interest group density, BUREAUCRACY = spending for Medicaid administration, URBAN = urban population.

The proxies for supply and demand are expected to influence Medicaid spending effort negatively. Higher proportions of less wealthy states' total populations have incomes at or near Medicaid eligibility cutoffs; but wealthy states, which generally are larger, have greater absolute numbers of Medicaid recipients, and thus have higher aggregate demand for care. Wealthy areas attract more physicians, increasing supply. Medicaid consumes a larger portion of state budgets in poor states. Thus we expect spending effort to decrease as the supply of, and demand for, Medicaid policy increase.

State political characteristics reflect economic development. Poor states tend not to have professionalized state governments. They tend to tax regressively, have low levels of political participation and competition, and tend to sponsor the least innovative or generous welfare programs. Economic development is a catalyst for political and policy development. The political systems in wealthy states, already better developed than those in poor states, continue to develop. Poor states' systems are hard pressed to catch up with those of wealthy states. We expect coefficients between measures of political competition and development to be related positively to the measure of policy effort. Three measures of state political characteristics—bureaucratic professionalism, interest group diversity, and political ideology—are incorporated to capture unique dimensions of each state's degree of political development.[3]

Bureaucracies influence complex policy areas through their control of expertise and information (Niskanen 1971; Thompson 1981; but see Miller and Moe 1983). The primacy of the states at Medicaid's inception underlines the importance of having a well-developed system for program administration. Responsibility for the interpretation of complex Medicaid eligibility policies, determinations of what constitutes appropriate care (and thus whether it will be paid), and other specialized decisions heighten bureaucratic power. We use the amount spent during 1982 for Medicaid administration as a proxy for bureaucratic professionalism. We expect it to influence the Medicaid effort indicator positively.

The diversity of a state's interest groups is also important in Medicaid policymaking. Decision makers seldom decide explicitly to ignore the needs of particular groups. Instead, they do what they can with what they have, and groups are excluded due in part to poor articulation of demand. Olson (1982) argues that the overall direction of state Medicaid policies reflects the demands of the majority, who are attuned only to "conspicuous and controversial choices" but that "smaller choices needed to implement [the] programs are influenced primarily by a minority of organized providers of care" (Olson 1982, 27–28). That all states now have Medicaid policies reflects the observation that the broad outlines of policies are passed to satisfy broad public demand. Few citizens know the proportion of their incomes spent to support Medicaid. The specifics of policies are determined largely by the group conflicts in a state.

Our measure of interest group diversity is based on Morehouse's (1981) three-tier categorization of interest group strength in the states. Guided by existing literature on the states, she catgorizes their group systems on a continuum ranging from strong groups (representing states with monolithic industries and poorly developed group systems subject to control by single groups) to weak groups (suggesting a multiplicity of groups representing diverse interests).[4] States with strong groups are coded one, moderate groups two, and weak groups three. We expect diverse interest groups to influence states to devote higher proportions of their budgets to Medicaid, as their diversity opens the door for customarily weak groups. This may work in a couple of ways. First, a diversity of groups may signal heightened power for welfare rights groups or others demanding broad welfare policy. Second, and possibly more importantly, providers' groups are more diverse where there are more groups in the aggregate. Diverse providers may demand different things from Medicaid, so the program may prove larger in terms of the pool of eligibles included, the types of services covered, and in the share of state income it consumes.

Finally, we expect state ideology to enter significantly into the Medicaid effort equation. The portion of personal income dedicated to Medicaid will increase with heightened ideological liberalism. State ideology is not especially easy to measure; we use as proxy for liberalism the average Americans for Democratic Action (ADA) legislative score for members of each state's congressional delegation. This is not an ideal measure. It addresses neither state policy issues nor state policymakers' voting records, but it is sufficiently sensi-

Table 1. The Determinants of Demand for Medicaid Services

Variable	Elasticity	Standard Error	T-Ratio	Approximate Probability > T
INTERCEPT	4.32	2.02	2.14	.04
#SUPPLY	1.21	.25	4.77	.01
PRICE	-.45	.27	-1.64	.11
URBAN	-.18	.21	-.90	.37
UNEMPLOYMENT	.87	.20	4.43	.01
#MEDICAID EFFORT	.18	.19	.94	.35

Note: $N = 49$; $DF = 43$; $R^2 = .94$; $F = 142.6$; $P > F = .01$. All variables are expressed as natural logarithms. See the appendix for variable sources and definitions.

tive to pick up variations among states that translate into state policy differences.[5]

To summarize, the model of Medicaid policy-making developed above is based on the assumption that there is a supply and demand for public policy. Socioeconomic measures are exogenous determinants of demand, supply, and policy. Measures of political characteristics are exogenous determinants of policy outputs.

Data Analysis

Models are estimated cross-sectionally using 1982 data.[6] We choose 1982 because several states engaged in Medicaid reform in response to OBRA 1981. The results reported are products of two-stage least squares estimation, which differs from ordinary least squares estimates inasmuch as they account for mutual causation among the endogenous variables. Estimation of a simultaneous system best fits the conceptual model developed above.[7]

All variables used are transformed to natural logarithms for two reasons. First, logging variables mediates heteroscedasticity in some distributions.[8] Second, parameter estimates between two logged variables may be interpreted as elasticities. An elasticity of one indicates that the dependent variable changes 1% with each percentage change in the independent variable. Thus the use of logged variables reduces the problem of heteroscedasticity and clarifies the meaning of the regression coefficients.

Demand for Medicaid services (Table 1) is affected most strongly by supply and unemployment. Demand for services increases 1.2% with each percentage increase in the supply of physicians, and almost .9% in the presence of a 1% rise in unemployment. Parameter estimates for supply and unemployment are statistically significant well under the .01 critical range.[9] The price variable coefficient is negative ($b = -.45$) and approaches statistical significance ($p = .11$), suggesting that price constraints demand for Medicaid services despite there being no real financial cost to the recipient.

The elasticity for the Medicaid effort coefficient is .18; demand increases less than .2% with each percentage increase in the proportion of a state's personal income devoted to Medicaid spending. Contrary to our expectations, urbanization's influence on demand is opposite that of the Medicaid effort variable ($b = -.18$), indicating that demand for services decreases almost .2% with each percentage increase in urban population.

Supply of services (Table 2) is most sensitive to income, followed in turn by Medicaid effort, price, and urbanization. The supply of physicians increases over .7% in the presence of a 1% increase in

Table 2. The Determinants of Supply of Medicaid Services

Variable	Elasticity	Standard Error	T-Ratio	Approximate Probability > T
INTERCEPT	7.23	1.81	3.99	.01
#DEMAND	-.04	.12	-.34	.74
PRICE	-.33	.17	-1.89	.07
#MEDICAID EFFORT	.34	.13	2.60	.01
URBAN	.27	.09	3.05	.01
INCOME	.73	.15	4.99	.01

Note: $N = 49$; $DF = 43$; $R^2 = .98$; $F = 339.4$; $P > F = .01$. All variables are expressed as natural logarithms. See the appendix for variable sources and definitions.

personal income. The effect of spending effort is not so pronounced: physician supply increases about .3% with each percentage increase in Medicaid effort. Contrary to our expectations, price has a polar opposite effect: supply decreases .3% with each 1% increase in hospital expenses per day. Physician supply increases about .25% with each 1% increase in urbanization. The demand elasticity ($b = -.04$) is neither statistically significant nor in the hypothesized direction.

Regression results for equation 6, in which Medicaid spending effort is the dependent variable, are contained in Table 3. Medicaid effort is most sensitive to supply: states' Medicaid spending efforts decline by .8% with each percentage increase in the supply of physicians. Consistent with the supply variable, the demand variable is associated negatively with the spending effort measure. As expected, the price variable is related positively to the Medicaid effort measure.

Each of the three political system measures enters the model significantly. Medicaid spending effort increases over .6% with each percentage increase in administrative funding. The interest group measure also exerts a significant impact: Medicaid effort increases as the interest groups in a state political system become more dense. Ideology also influences the degree to which a state devotes resources to paying for Medicaid. Medicaid spending as a proportion of states' total personal income increases by about .17% with each percentage increase in political liberalism. Finally, Medicaid spending

Table 3. The Determinants of Medicaid Effort

Variable	Elasticity	Standard Error	T-Ratio	Approximate Probability > T
INTERCEPT	-1.13	2.64	-.43	.67
#SUPPLY	-.80	.42	-1.92	.06
#DEMAND	-.23	.29	-.78	.44
PRICE	.12	.31	.38	.71
IDEOLOGY	.16	.06	2.65	.01
GROUPS	.35	.12	3.06	.01
BUREAUCRACY	.63	.25	2.56	.01
URBAN	.38	.26	1.44	.16

Note: $N = 49$; $DF = 41$; $R^2 = .49$; $F = 5.74$; $P > F = .0001$. All variables are expressed as natural logarithms. See the appendix for variable sources and definitions.

effort increases nearly .4% per percentage increase in urban population.

Discussion

The empirical results support our characterization of policy as an outcome of market influences, reflecting the effect of the peculiarities of the health services market on public spending effort. First, they demonstrate that the supply of medical services affects both demand and Medicaid spending effort strongly. Increasing the number of physicians in a state by 1% leads to a greater-than-1% rise in demand for services. Despite this higher demand for services, Medicaid spending effort declines precipitously in the presence of high numbers of providers. Hanson (1984) reports that the eligibility and service coverage of state Medicaid policies increase along with the number of physicians. Taken together, this implies that high spending effort is not associated with good service coverage. States exerting greater effort get worse returns than states spending relatively less.

In a seemingly contrary result, our results show that the supply of services increases with spending effort. The negative influence of supply on spending effort is immediately confounding, but makes sense upon further consideration. States that spend relatively high proportions of personal income on Medicaid are primarily poor states with correspondingly ill-developed health sectors. There are fewer physicians where the proportion of spending is highest. Physicians are drawn to wealthy areas, so areas with high incomes have more physicians and devote lower proportions of total income to Medicaid.

Consistent with our expectations, the sign of the price variable is positive in the Medicaid effort equation. Price measures variation in states' costs of providing access to care. In two states with identical amounts of personal income and identical Medicaid programs, higher costs of care lead necessarily to higher proportions of personal income being spent on Medicaid. Under conditions of physician or hospital bed oversupply states may have the latitude to set reimbursement rates well below the usual fee as result of weakened provider interest groups.

Treatment of demand as endogenous to the system of equations best reflects our understanding of theories of health markets. These results indicate that demand for Medicaid-paid health care significantly affects neither the supply of services nor the relative Medicaid spending efforts of states' citizens. These findings suggest that supply of services is the driving force in determining both demand and spending effort, although it affects them in opposite ways, driving the former up and the latter down. Demand is most sensitive to measures of supply and unemployment and insensitive to the policy indicator, reflecting the widespread notion that spending for the program is out of control.

The positive influence of political system traits reflects the redistributive aspects of Medicaid policies. Consistent with our expectations, ideology, interest group diversity, and administrative professionalism influence Medicaid effort positively. Their influence, taken with the negative effect of supply, suggests an independent influence of state political characteristics on redistributive policy decisions. Operating within socioeconomic constraints and federal guidelines, states' spending efforts are influenced independently by ideology and political development.

State efforts for providing health insurance to the indigent population are conditioned by political, market, and socioeconomic factors. Increases in the number of providers leads to decreases in spending effort. Higher spending effort increases supply. Where interest groups are most dense there is higher Medicaid spending effort. According to the logic of

Olson (1982, 1985), this is to be expected: organized interests exert direct influence on policy decisions. In a related vein, the health care bureaucracy takes on characteristics of a captured regulatory agency as described by Stigler (1968), Mitnick (1980), and others. From that perspective there is no need for the Medicaid recipient population to demand generous Medicaid policy in the presence of a large provider sector. Their interests are protected because they converge with providers' interests. The incorporation of political and economic components in the model provides a more thorough explanation for differences in state Medicaid effort than is gained through the exclusive use of either approach.

Conclusion

The results reported herein have both substantive and methodological implications. Substantively, they suggest that large portions of state and national budgets are at the mercy of a relatively small and well-organized interest. This is not especially surprising. Medical associations have successfully blocked various national health insurance plans (Poen 1979), influenced the design of Medicaid and Medicare (Olson 1985; Starr 1982), and protected members' economic interests by strategically donating funds to house members sitting on committees with jurisdiction over health care regulatory issues (Keiser and Jones 1986). Our finding that states with lower numbers of providers spend higher proportions of personal income on Medicaid may be interpreted in at least two ways. First, and most charitably to providers, it may indicate that poor states by definition have lower amounts of personal income and higher percentages of persons in need of state-supported health insurance, thus increasing Medicaid effort. Another interpretation, supported by the finding that Medicaid effort increases as the number of providers decreases, is that relatively smaller provider groups are better able to influence policy than larger, more diverse, groups, as are found in wealthier states. Where there are large numbers of providers the health care industry is sufficiently fragmented by recent changes in the organization and delivery of health services that neither the industry as a whole nor its parts—physicians, hospitals, nursing homes, and so on—constitute monolithic interests that can reasonably be expected to speak with a unified voice.

Physicians are fragmented along the lines of specialization, form of practice (e.g., private office, group, prepaid, etc.), and other characteristics. Hospitals, where most of the money goes, are also divided along several dimensions. Interests of charity hospitals differ from those of for-profits, rural hospitals' needs differ considerably from urban hospitals', teaching hospitals' needs differ from nonteaching hospitals', and so on. Small, unified interests are more effective than large, diverse interests (Olson 1965, 1982). As a state's health care providers become more diverse their ability to influence policy decreases.

The strength of individual health interest groups also has been eroded by the growing strength of insurance companies, whose interests are often opposed directly to those of provider groups. Blue Cross and Blue Shield, established during the 1930s as financing adjuncts of the hospitals and physicians, respectively, once controlled the majority of the health insurance market and have seen that share diminish rapidly during the past decade. Neither Blue Cross nor Blue Shield does the bidding of its respective patron organization as once was the case. More threatening to providers' groups are proprietary insurers, who confront providers more openly on questions of cost containment. Providers are being forced to accept the role of private and not-for-profit insurers in determining appropriate forms of treatment and payments for particular episodes of illness. Medicaid programs may simply be lagging behind their private sector equivalents and may in the future exert a greater influence over spending outcomes.

A diversity of interest groups, especially in the presence of liberal ideology and a well-funded bureaucracy, may lead to higher amounts of income being used to purchase health care services for purely altruistic reasons. However, Medicaid may simply be a case in which the interests of the indigent population and the health care industry converge. With few groups the physicians' and hospital lobbies are likely to be less fragmented and, because expansion of Medicaid opens the medical market to numerous types of providers, may be biased toward having a fairly restrictive program. In that case physicians may opt to discriminate against other types of providers, choosing control over a small pie to competition for a slice of a larger one.

These findings also indicate that citizens living in relatively more developed states get proportionately more from Medicaid spending than persons in poorer states. Medicaid recipients receive better access to care in states with more providers and pay a lower proportion of personal income than they would for worse access in a poorer state. Ironically, conservative citizens who wish to hold down access to Medicaid for ideological reasons may be better served economically by living in a wealthier, more liberal area. There a smaller portion of their income is spent for welfare medicine, which may better reflect their preferences. Federal financial contributions, designed to equalize programs among states, are not achieving that goal; citizens in poorer states bear a larger Medicaid burden than they would if they lived in a wealthier area.

These results also point to what may be a promising avenue for future comparative policy research. Although applied only to Medicaid, the approach used herein may be incorporated into a more general model of policy determination. Relationships in the Medicaid policy environment may be likened, for example, to relationships that affect governmental dealings with the road-building industry. In each case, neither providers (i.e., physicians or construction companies) nor recipients (i.e., Medicaid eligibles or parties to be served by new roads) need be entirely satisfied with the policy enacted, but the groups' demands are joined by a common bias toward program expansion. Medicaid eligibles want more services. Providers want an expanded market. The public may be unwilling to bear the costs of expanded service but is not easily mobilized given their general disorganization and apathy. Medicaid's reliance upon private providers ensures physicians' groups', hospital associations', and other health care industry's interests a prominent role in determining the scope of a state's Medicaid program. Eligibles receive more expansive services as government meets the demands of providers. Government's role in the program is to monitor and control spending; but the political costs of doing so may outweigh the fiscal gains, thus providing few political incentives.

The empirical evidence supports portions of the market model of policy determination. This is consistent with the progression of comparative policy outputs research. The aim of early comparative policy research (e.g., Dawson and Robinson 1963; Dye 1966; Hofferbert 1966) was to test competing explanations for differences in state spending, one based in public finance economics (see, e.g., Fabricant 1952), the other deduced from the work of political scientist V. O. Key, Jr. (1949).[10] More recently, researchers have moved beyond debates addressing the primacy of either economic or political explanations for policy differences to the synthesis of political and economic models of governmental policy decisions (see, e.g., Chubb 1985; Godwin and Shepard 1976; Hanson 1983, 1984; Plotnick and Winters 1985). This synthesis has done much to aid in developing theories of governmental policy decisions: Charles Jones' once-apt observation that the cumulative theoretical contribution of the literature was "like taking a long trip only to find yourself back home again" (1973, 34) no longer holds. We think the road is now fairly clearly marked.

Appendix: Variables and Sources

SUPPLY: Total nonfederal physicians, 1982. Source: American Medical Association, *Physician Distribution and Medical Licensure in the United States* (Monroe, LA: American Medical Association, 1984).

DEMAND: Total unduplicated Medicaid recipients, fiscal year 1982. Source: Health Care Financing Administration, HCFA 2082 data files (Washington: U.S. Department of Health and Human Services, 1982).

MEDICAID EFFORT: State Medicaid vendor payments as a proportion of total personal income, 1982. Source: Authors' calculations, Health Care Financing Administration, HCFA 2082 data files (Washington: U.S. Department of Health and Human Services, 1982); and U.S. Bureau of Census, *Statistical Abstract of the United States.* 104th ed. (Washington: U.S. Bureau of Census, 1983).

PRICE: Inpatient expense per day, 1982. Source: American Hospital Association, *Hospital Statistics* (Chicago: American Hospital Association, 1983).

UNEMPLOYMENT: Total unemployment, 1982. Source: U.S. Bureau of Census, *Statistical Abstract of the United States.* 104th ed. (Washington: U.S. Bureau of Census, 1983).

URBAN: Total number of persons in metropolitan areas, 1982. Source: U.S. Bureau of Census, *Statistical Abstract of the United States.* 104th ed. (Washington: U.S. Bureau of Census, 1983).

INCOME: Total personal income, 1982. Source: U.S. Bureau of Census, *Statistical Abstract of the United States.* 104th ed. (Washington: U.S. Bureau of Census, 1983).

IDEOLOGY: State congressional delegation average Americans for Democratic Action (ADA) legislative score, 1982 (ranges from zero to one hundred). Source: Americans for Democratic Action, *ADA World* 40 (1983).

GROUPS: Morehouse (1981) index of group strength.

BUREAUCRACY: Total state Medicaid administrative expenditures, 1982. Source: Health Care Financing Administration, 1984. Medicaid Program Characteristics data files (Washington: U.S. Department of Health and Human Services, 1982).

Notes

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1. The simple correlation (r) between the Medicaid effort measure and total spending is .31.

2. Thus recipients enter a principal-agent relationship as described by Moe (1984), with government acting as monitor.

3. With these measures we seek to capture three dimensions of state political systems: professionalism, competitiveness, and ideological predisposition. (For a discussion of the dimensionality of state

systems, see Sharkansky and Hofferbert 1969.) Other measures may be substitutable, but we consider these most appropriate for the case at hand. We think the interest group measure is superior, conceptually, to a measure of partisan competition. As has been noted elsewhere, partisan competition is a function of diversity within states (Barrilleaux 1986; Patterson and Caldeira 1984; Sullivan 1973). Further, states with low interparty competition have been shown to be internally competitive (Jennings 1976; Wildgen 1974), suggesting that interparty competition is not necessarily a sound measure of the competitiveness of political systems. Political parties are simply one of the cures for the problems arising from faction noted by James Madison in the *Federalist Papers*; the diversity of interest groups is closer to the heart of the matter.

4. Morehouse's categorization of group strength in the states is based upon her fairly impressionistic reading of materials pertaining to the subject within the states. To validate her measure, she reports simple correlations between her categorical measure and an updated Hofferbert integration index (Morehouse 1981, 113). The distribution of weak group states (i.e., numerous groups) correlates strongly and positively with states with high integration scores (r = .87). The distribution of strong group states (i.e., few groups) correlates negatively and strongly with the distribution of states scoring low on the integration measure ($r = -.57$). The integration index is designed to measure a state's social and tertiary economic development. Integration indicates diversity, as does diffuse group power. Hence we present the Morehouse measure as a suitable measure of the diversity of interests within the states.

5. The measure of state citizens' ideology developed by Wright, Erikson, and McIver (1985) appears to have greater face validity than the ADA scores indicator used herein. Briefly, Wright, Erikson, and McIver's measure is constructed by aggregating 1976 through 1982 CBS-*New York Times* poll responses to an ideological self-evaluation question. Index scores are calculated for 47 contiguous states and for a pseudostate constructed from District of Columbia responses. They construct two indexes from the survey data—a "general population" measure and an "active electorate" measure—each of which was tested in our model. The survey-based measures have little effect on parameter estimates of ideology's effect on Medicaid effort. Using the Wright, Erikson, McIver "active electorate" measure in the Medicaid effort equation, $b = -.15$, $t = -2.26$. The parameter estimate with the ADA measure ($b = .16$, $t = 2.6$) is remarkably similar. Signs are reversed because higher scores on the ADA measure indicate a more liberal ideology and high scores on the survey measure represent less liberal ideology. Other relationships in the system are stable, although the explanatory power of the model increases when the survey measure is used.

Given this, we use the ADA measure for two reasons. First, it is available for each of the 49 states that had Medicaid programs during 1982. We think it more valuable to maintain cases, especially when the empirical gain of using the survey-based measure is marginal, especially given our use of simultaneous estimation methods. Second, the ADA measure, though not directed explicitly at state policy issues, contains some policy-relevant information inasmuch as it reflects voters' willingness to send "liberal" members to Congress. The survey measure contains none. Our findings are consistent with those of Holbrook-Provow and Poe (1987), who report little difference in the empirical performance of roll call measures of state ideology and the Wright, Erikson, and McIver measure.

6. Our data cover the 49 states that had fully implemented programs during 1982. The excluded state, Arizona, began implementing a demonstration program during the early 1980s.

7. For discussions of simultaneous equations methods, ranging from practical to somewhat less so, see Gow and White 1986; Kennedy 1984, 106–26; Maddala 1977, 470–92; and Pindyck and Rubinfeld 1981.

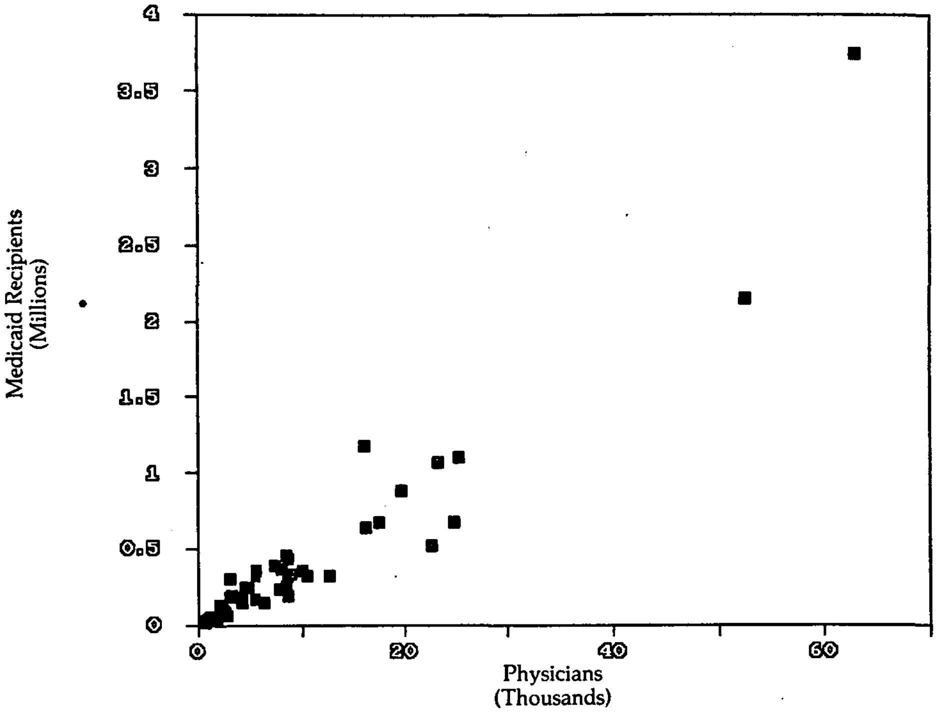
8. Heteroscedasticity occurs when the variances of error terms are not equal for each observation. It is often a problem with aggregate data research, especially when the size of units of analysis varies widely, as with state data. The figures below illustrate the mediating effect of logging variables.

Figure 1, in which raw values of physicians are plotted against raw values of Medicaid recipients, is a heteroscedastic distribution. When values are converted to natural logs (Figure 2), the distribution assumes a linear form.

9. We are sensitive to arguments against the use of tests of significance when using a population, as is the case with this research. One critic pointed out to us that "God and Eisenhower" gave us only 50 states and, more importantly, argued that good policy decisions may be ruled out based on arbitrary decision rules. Though we accept these problems, we consider statistical tests useful evidence in evaluating research. Further, compelling conceptual and statistical arguments for the use of significance tests in population-based research remain. Conceptually, a cross-section of states constitutes neither a closed theoretical nor a closed population system (Blalock 1967, 131–32). For a theoretical system to be closed, no variables may have been omitted and mathematical functions must be perfect with no random or error terms (Blalock 1967, 131). Empirically, statistical tests are important when used to assess the reliability of estimates. The standard errors of regression coefficients may be viewed as evidence of the reliability of estimates rather than as tests of the null hypothesis (Blalock 1967; Schneider and Darcy 1984; Winch and Campbell 1970).

10. Much of the "politics-versus-economics" de-

Figure 1. Plot of Unlogged Values



bate seems to be based on loose readings of Key's *Southern Politics* (1949). Jones (1973) documents widespread incorrect citation of putatively seminal passages, suggesting that scholars relied on their forebearers' interpretations rather than the original. More conclusively, Uslaner (1978) argues that Key's research provides little theoretical justification for widely tested hypotheses regarding the effects of party competition and legislative malapportionment on the policy outputs of democratic governments.

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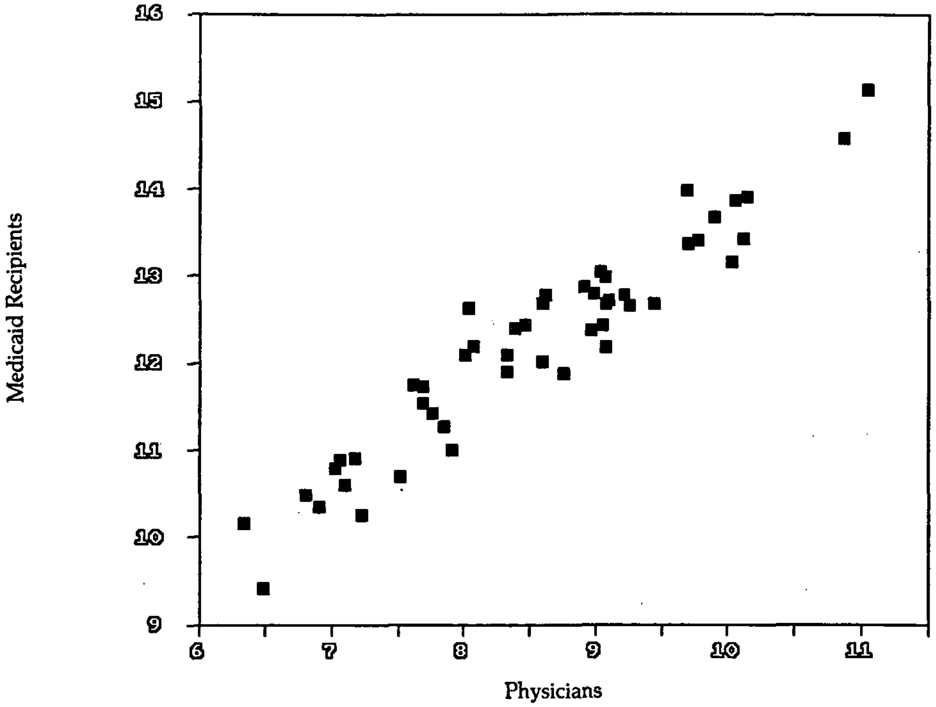
Figure 2. Plot of Logged Values Using Natural Logarithmic Function



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State Medicaid Policy

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ORGANIZED INTERESTS AND AGENDA SETTING IN THE U.S. SUPREME COURT

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Participation as amicus curiae has long been an important tactic of organized interests in litigation before the U.S. Supreme Court. We analyze amicus curiae briefs filed before the decision on certiorari and assess their impact on the Court's selection of a plenary docket. We hypothesize that one or more briefs advocating or opposing certiorari increase the likelihood of its being granted. We test this hypothesis using data from the United States Reports *and* Briefs and Records of the United States Supreme Court *for the 1982 term. The statistical analysis demonstrates that the presence of amicus curiae briefs filed prior to the decision on certiorari significantly and positively increases the chances of the justices' binding of a case over for full treatment—even after we take into account the full array of variables other scholars have hypothesized or shown to be substantial influences on the decision to grant or deny.*

Each year a multitude of interest groups participate in various guises before the Supreme Court. It is clear that in the last couple of decades interest groups have stepped up their activities before the courts, just as they have in other political arenas (Bradley and Gardner 1985; O'Connor and Epstein 1982; Schlozman and Tierney 1986; Walker 1983). Interest groups can of course choose to take part before the Supreme Court in many different roles, and they most often do so as amici curiae. Organized interests have long participated actively as amici curiae for decisions on the merits. But political scientists, with few exceptions, have not measured the impact of these briefs, and no one has done so for decisions on writs of certiorari and appeal.

We contend that not only are amicus curiae briefs important, but that the decision to review a case ranks as important as—if not more important than—the decision on the merits. In setting its agenda, the Supreme Court each year determines new winners and losers in the struggle for economic, political, and social power. We believe, therefore, that powerful interests will not stand by passively as the Supreme Court legitimizes literally hundreds of decisions in the lower courts each year and calls many others into question. And so, as Justice Brandeis once remarked, "the most important thing the Court does is not doing." Thus we ask, To what extent does amicus curiae participation by organized interests influence the Supreme Court's selection of cases for plenary hearing? We address this question within a more general analysis of other relevant forces that impinge upon the Court's choices in gatekeeping.[1]

Here we present the first of our empirical results on the impact of amicus curiae participation on decisions to grant writs of certiorari during the 1982 term of the Court.[2] These data come from the *United States Reports* and *Briefs and Records of the United States Supreme Court.* For each case we have determined whether or not amicus briefs were filed *before* the decision on certiorari or appeal; which, if any, organization filed; the presence of the federal government as a party or of "conflict"; dissents below; ideological outcome in the lower court; the type of case at hand; the Court's decision; and a variety of other relevant variables. The statistical analysis demonstrates that the presence of amicus curiae briefs significantly and positively increases the chances of the justices' binding a case over for full treatment—even after we take into account the full array of variables that other scholars have hypothesized or shown as substantial influences on the decision to grant or deny.

We organize the discussion and analysis into six sections. After presenting a brief overview of organized group activity in the judicial process, we describe our theoretical perspective and present our primary hypotheses. We then review various other explanations for certiorari analyzed in empirical studies over the years. We describe our data and present the results of our statistical analysis. Finally, we conclude with some general observations on the role of groups in the judicial process and describe some of our plans for future research.

Organized Interests in the Judicial Process

The idea that interest groups have significance in litigation before the Supreme Court goes back at least as far as Bentley's *Process of Government* (1908): "So far from being a sort of legal machine, courts are a functioning part of this government, responsive to the group pressures within it, representatives of all sorts of pressures, and using their representative judgment to bring these pressures to balance, not indeed in just the same way, but on just the same basis that any other agency does" (p. 393; see also Truman 1951, 479–99; Peltason 1955). The tactics groups can use to influence legislation are considerable, including "class actions" and test cases; participation as amici curiae; giving of advice and service; expert testimony, and financial assistance; and taking control of the law suit (see Barker 1967; Hakman 1966). Vose (1959) blazed a trail with his meticulous and intriguing case study of how the NAACP Legal Defense Fund used a variety of strategies and tactics in its campaign of litigation to end the restrictive covenant in housing (see also, e.g., Manwaring 1962 and the citations collected in Vose 1981).

Interest groups, of course, vary enormously in the extent to which they choose to use the courts in a direct fashion. On the basis of a study of the 1958–64 terms of the Court, Hakman (1966, 47) concluded that the "actual judicial process appears to be a close approximation of the traditional legal model in which judicial policymaking emerges through ad hoc private controversies. . . . Organized interest groups would appear to play a relatively minor role in Supreme Court decision-making" (p. 50). He went on to state in a later paper that "most 'established' or 'permanent' organizations had little or no connection with the judicial process" and seldom used litigation to achieve their goals (Hakman 1969, 245). Nonetheless, despite Hakman's objections, scholars have demonstrated over and over again the vigorous, extensive, and continuing efforts on the part of interest groups to lobby the courts (see O'Connor and Epstein 1983a; Olson 1981;

Schlozman and Tierney 1986, 358–85; Vose 1981). At least since the 1960s evidence of the participation of interest groups as amici curiae or sponsors has virtually leaped from the pages of the *United States Reports* even on the most cursory inspection.

Although the filing of amicus curiae briefs is just one of many tactics available to organized interests,[3] participation as amicus has figured as an indicator of interest group activity in virtually every pertinent study. More important, we have good reason to believe that such filings are consequential. The filing of an amicus brief, apart from the quality or persuasiveness of the arguments presented, provides the justices with an indication of the array of social forces at play in the litigation. In *Baake* v. *Regents of the University of California*, for example, the Court had 57 briefs from "friends" and without doubt obtained a vivid picture of whose interests were at risk. The Supreme Court, as Krislov has remarked, sometimes views the amicus as a "potential litigant in future cases, as an ally of one of the parties, or as the representative of an interest not otherwise represented" (1963, 704). Even though Hakman (1969) deprecated the importance of interest groups, he reported filings of amicus briefs in nearly 20% of the decisions on the merits between 1928 and 1966 (for a similar figure, see Puro 1971). O'Connor and Epstein (1982) validate the percentage Hakman (1969) and Puro (1971) reported for 1928–1966 and then go on to show that the percentage of cases in which amicus briefs appear has risen to more than half in the last decade.

So, clearly, interest groups have used the vehicle of amicus briefs often and, in recent years, with increased frequency. There is, in addition, some fragmentary evidence that participation as an amicus curiae constitutes an efficacious route for interest groups to take in their attempt to influence the Court. Puro (1971) found that groups on the "liberal" side participated more often as friends of the Court and were more likely than conservatives to triumph on the merits (pp. 254–55).

We, too, believe that amici curiae make a difference in litigation. And we propose that their impact will be apparent at the agenda-setting phase.[4] The presence of amici during case selection communicates to the Supreme Court information about the constellation of interests involved, and this information, we suggest, is both valued and heeded by the justices and their clerks.

A Theory of Case Selection

Theoretically, we propose that justices of the U.S. Supreme Court are motivated by ideological preferences for public policy and that they pursue their policy goals by deciding cases with maximum potential impact on political, social, or economic policy (see Murphy 1964; Rohde 1972; Rohde and Spaeth 1976). Chief Justice Rehnquist has spoken words consistent with this notion: "There is an ideological division on the Court, and each of us has some cases we would like to see granted, and on the contrary some of the other members would not like to see them granted" (quoted in O'Brien 1986, 160). This is not, of course, to say that the justices act without other constraints. We recognize that the justices maximize their positions subject to a complicated matrix of legal rules, social norms, and institutional expectations. Nevertheless, the efficient pursuit of policy objectives implies that justices will devote their resources to those cases that have the most profound implications for policy relative to the effort required to hear and to decide the cases. In cases where a justice agrees with the decision of the lower court there is little incentive to alter the status quo, but the costs of reviewing the case may well be considerable, as the time constraint on justices is enormous (see Baum 1977).

We predict that justices will review the cases they wish to reverse on ideological grounds and the cases they think will have the greatest practical significance.[5] Normally, of course, the ideological issues at stake in a case are apparent upon a close reading of the petition for certiorari and the brief in opposition. But the practical impact of a case can be quite difficult to assess. Thus, in our theory, we focus on the process by which the justices learn about the practical significance of cases brought before the Supreme Court.

Quite simply, our theory assumes that the potential significance of a case is proportional to the demand for adjudication among affected parties and that the amount of amicus curiae participation reflects the demand for adjudication. We propose that amicus curiae participation by organized interests provides information, or signals—otherwise largely unavailable—about the political, social, and economic significance of cases on the Supreme Court's paid docket and that justices make inferences about the potential impact of their decisions by observing the extent of amicus activity. Although potential amici have the option of filing briefs either in support of, or in opposition to, petitions for writs of certiorari, our theory suggests that both types of briefs should have the same effect—that is, increase the likelihood of a grant. The reason is simply that both kinds of briefs draw attention to the potential significance of a case. Consequently, the mere presence or absence of a brief amicus curiae may weigh in more heavily than the substantive arguments presented.

In this way amicus curiae activity assists the justices in winnowing down the set of plausible cases to an even smaller set of cases that satisfy their own individual ideological agendas. Naturally, even this subset of cases will be larger than what the Court can accommodate during a given term, and we recognize that further winnowing criteria must be used. Nevertheless, we predict that cases within this smallest set will have an unusually high probability of being accepted for full review.

That participation as amicus curiae is costly to an organized interest is an implicit but necessary assumption of our theory. Only if the preparation and filing of a brief amicus curiae is costly can a brief function as a reliable signal about the importance of a case; otherwise a group might very well file one in each and every case. Naturally, this situation of costless participation would communicate no discriminating information at all to the members of the Court. So far as we can determine, an amicus brief on a petition for certiorari does indeed involve a nontrivial expense. From the general counsels of several large organizations with headquarters in Washington, DC we have obtained estimates of the external costs of preparing an amicus brief on a petition for certiorari. Their estimates consistently fell in the range between $15 thousand and $20 thousand for a single brief prepared by a reputable law firm. One suggested that he had paid as much as $60 thousand. Thus, even organizations with a large budget for litigation cannot and do not file amicus curiae briefs with reckless abandon.

The importance our theory places on the informational value of amicus curiae briefs implies that the rules and procedures of the Supreme Court should encourage—or at least not impede—the filing of briefs and that practitioners should have some awareness of the importance of filing briefs (see O'Connor and Epstein 1983a on changes in the rules). Even though the Court officially does not look with favor on amicus briefs filed prior to the decision on whether or not to grant certiorari or note probable jurisdiction (see Rule 36.1), the Court, in fact, rarely denies motions for potential amici to participate at this stage of the litigation. Potential amici have 30 days from the date on which the respondent receives the petition for writ of certiorari or jurisdictional statement to file a brief for or against certiorari or jurisdiction. To qualify as an amicus curiae, a nonparty must show that it has a substantial interest in the legal question and must file a motion and brief in a "timely" fashion. Governments may of course participate as amici without leave of the parties (Rule 36.4). Others may do so with the permission of the parties. If the parties refuse to permit a nonparty to submit a brief, the hopeful amicus may file a motion with the Supreme Court for leave to submit a brief. An organization or individual seeking to participate as amicus curiae will usually submit a brief at the same time it files a motion for leave to file a brief. The Court then acts on both the motion and the brief (see, generally, Stern, Gressman, and Shapiro 1986).

Not only are briefs relatively easy to file from a procedural standpoint, but practitioners recommend doing so. It makes good sense, according to one practitioner, to file a brief prior to certiorari even if the Court denies the motion. The Court, after all, has to read the brief to find out whether the motion is meritorious. The leading manual of practice in the Supreme Court states that "where there is doubt, as there usually is, that a petition will be granted, statements by amici which show that the case is generally important can be of significant aid to the petitioner" (Stern, Gressman, and Shapiro 1986, 391; see also Ennis 1984). These scholars go on to add that "indeed, an amicus brief that demonstrates that the case is important to others besides the immediate parties may be more helpful at this stage than after certiorari has been granted" (Stern, Gressman, and Shapiro 1986, 395; see also Baker 1984). But they and others advise that nonparties who do not favor jurisdiction should refrain from filing a brief amicus curiae against a grant of certiorari. A brief of this sort may simply call attention to a case. One scholar-practitioner boldly states, "Never file an amicus curiae brief against certiorari" (Shapiro 1984, 24). Such advice implies, as does our theoretical perspective, that the primary informational value of amicus briefs lies in their presence or absence alone, not in the direction of the legal arguments advanced within.[6]

Because of the confidentiality of the conference and individual deliberations, we have no means of knowing with certainty how the justices view and value briefs amicus curiae prior to certiorari. Still, evidence in the private papers of the justices suggests that at least some of the justices some of the time pay attention to the presence, identity, and positions of amicus curiae for or against certiorari or jurisdiction.[7] For example, in a memorandum on *Citizens Publishing Company et al.* v. *United States,* Chief Justice Warren's clerk remarked,

> Two amicus briefs have been filed urging that the Court N[ote] P[robable] J[urisdiction]. The amici are publishers and individual papers operating in towns where papers operate under joint agreements which merge production, advertising, and circulation. The amici argue that this case has significance for the metropolitan newspaper industry, an industry in which it has become increasingly difficult for papers to operate without joint agreements. The appellants and amici present detailed arguments urging that it is not financially feasible for many papers to operate if the present decision stands. . . . It appears that the question is clearly substantial and it seems the Court should NPJ. (no. 243, 1968 term)

We could, without difficulty, present numerous examples of communications of this sort during both the Warren and Burger Courts.[8]

Our theoretical perspective gives us confidence that amicus curiae briefs and the ideological outcome in the lower court are important variables in justices' calculations about certiorari. We make no claim, though, that amicus briefs and ideological considerations exhaust the list of important predictors of certiorari. Previous research has revealed several other variables that appear to carry considerable weight in certiorari decisions, and we must take these forces into account in the subsequent test of our hypotheses. We turn now to a brief review of these findings.

Explanations of Gatekeeping in Previous Research

Since the passage of the Judiciary Act of 1925 the Supreme Court has for all intents and purposes functioned as a court of discretionary jurisdiction. Rule 17 (formerly 19) sets out the technical criteria for acceptance of a case via a writ of certiorari: the presence of important issues the Court has yet to decide, conflict among the courts of appeals on an issue, disagreement between the lower court and the Supreme Court's precedents, and deviation from the "accepted and usual course of judicial proceedings" in the court below (Stern, Gressman, and Shapiro 1986, 193–253). These desiderata, of course, only establish a starting point for the justices in their search.

Empirical evidence strongly suggests that Rule 17 does not explain how the Court makes gatekeeping decisions. Tanenhaus and associates (1963) discovered that reasons mentioned in Rule 17 appear in only two-thirds carried over for decision. Rather than simply applying Rule 17, Tanenhaus argued, the justices scan the petitions for a writ of certiorari in quest of readily identifiable cues. Justices, like other public officials, feel the pressure of work and the lack of time and so develop routines and shortcuts to achieve their goals. Using the *United States Reports*, Tanenhaus and his colleagues assessed the effects of four potential cues: presence of the United States as a party, presence of a civil liberties issue, dissension in the courts below, and an economic issue. These scholars found that all of the cues with the exception of economic issues increased the probability of the Court's taking the case.

Mining the data in Justice Burton's papers, Ulmer, Hintze, and Kirklosky (1972) studied the petitions that the chief justice had not placed on the "deadlist" and that thus had been discussed in conference. They reported that of the several cues, only the United States as a party made a difference in the Court's decision to grant or deny certiorari. Provine (1980)—also working with Justice Burton's papers—sought to controvert Tanenhaus and his associates. She argues that the patterns of voting do not support "cue theory" (p. 80): "The justices do indeed seem to act on certain shared ideas about the proper work of the Supreme Court, ideas that correspond with the responsibilities of the court of last resort in any complex legal system" (p. 103). Nevertheless, Provine's own results suggest a significant relationship between the variables Tanenhaus had identified and the decision to grant review.

More recently, Ulmer (1983, 1984) has persuasively shown that conflicts between the circuits and conflicts between the lower court and Supreme Court precedent dramatically raise the probability of the justices' placing the case on their agenda; and "genuine," as opposed to "alleged," conflict apparently motivates the Court (see also Feeney 1975). Other scholars have contended that the justices take into consideration not only attributes of the case but also the direction of the lower court's decision. The justices, in a word, take the case in order to reverse it in accordance with their preferences. Thus, for example, Armstrong and Johnson (1982) found that the Burger Court was more likely to grant a writ when the lower court's decision favored civil liberties than when the decision went against the claim of individual rights (see also Palmer 1982;

Provine 1980; Songer 1979; Teger and Kosinski 1980; Ulmer, Hintze, and Kirklosky 1972; for macro-level analyses see Caldeira 1981 and Likens 1979; for a process model, see Perry 1985, and Perry 1986).

Quite apart from the ideological alignments of the justices, the presence or absence of conflict between or within lower courts has proven useful for explaining the granting or denial of certiorari (Tanenhaus et al. 1963; Ulmer 1984). When the appellate court immediately below has reversed the lower court, the Supreme Court is more likely to grant certiorari, and when one or more judges in the court immediately below have dissented, the justices on the Supreme Court have, ceteris paribus, evinced a greater propensity to grant. The justices themselves will on occasion mention a dissent from below. More important for our purposes, in their memoranda on petitions law clerks regularly note who has written majority and dissenting opinions in the lower court and whether a reversal has occurred.[9] Both of these indicators—dissent and reversal below—signal ferment in the lower courts and suggest a problematic outcome, one perhaps worthy of a closer look.

Several scholars have demonstrated that as the Supreme Court chooses its plenary docket, it exercises biases in favor of or against certain kinds of issues. Thus, for example, Tanenhaus and his colleagues reported a significant relationship between the presence of a "civil liberties" claim and gatekeeping decisions (see also Provine 1980). We distinguish between "civil liberties" and other kinds of cases and test for the possibility that the Supreme Court is differentially sensitive to petitions based on these claims.[10]

Research has also demonstrated that when the federal government petitions as a party for a hearing, the Supreme Court is significantly more likely to grant certiorari than under other circumstances. The solicitor general, as others have demonstrated, maintains a special relationship with the Supreme Court; in fact, some refer to him as the "Tenth Justice" (Caplan 1987; Scigliano 1971; Segal 1988; Ulmer and Willison 1985). For a variety of reasons, he and his office have over the years maintained a high winning percentage as petitioners seeking certiorari and on the merits. For one, the solicitor general carefully screens cases for good vehicles and refuses to bring others. For another, because of the long-term stability of the staff, the Office of the Solicitor General constitutes a classic instance of a "repeat player," with all of the advantages attendant on that status (see Galanter 1974). (But see Caplan 1987 for a suggestion that recent solicitors general have squandered some of those advantages.)

On the one hand, the previous work on agenda setting in the Supreme Court impresses us as empirically sound, cumulative, and statistically sophisticated. On the other hand, we worry about the excessive and exclusive reliance on "cue theory" and the inability of existing models to account for much of the Court's agenda. Quite clearly, the Court is not bound by custom or procedure to hear only "conflictual" cases or cases in which the federal government is a party (e.g., Estreicher and Sexton 1986). In fact, a reasonable argument could be advanced that Supreme Court justices have routinely resisted imposing more restrictive procedures for the selection of cases in order to respond easily to changing political and economic demands. Consequently, we think models of the certiorari decision could profit greatly from consideration of preferences and demands of outside interested parties as revealed through amicus participation.

Statistical Analysis

Our primary hypothesis is that the greater the number of amicus curiae briefs filed for a given case—either in favor of, or in opposition to, certiorari—the greater the likelihood that the case will be granted a writ of certiorari. We test this hypothesis using data we have collected on all paid cases in which the Supreme Court granted or denied a writ of certiorari or affirmed, dismissed, reversed, or noted probable jurisdiction on a writ of appeal during the 1982 term (N = 2,061).[11] Of these 2,061 cases, 1,906 involved petitions for writs of certiorari, and the rest were decided on appeal. We confine our analysis to petitions for writs of certiorari, of which 145 (7.6%) succeeded. The dependent variable is whether or not the Supreme Court granted certiorari and the parties submitted briefs on the merits.[12]

A simple bivariate analysis of decisions on certiorari and filing of amicus briefs indicates considerable preliminary support for our main hypothesis. At least one brief was filed in 148 of the 1,906 petitions (108 cases with briefs in support only, 29 cases with briefs in opposition only, and 11 with briefs both for and against), and the Court granted certiorari in 54, or 36%, of these cases. In contrast, when no one filed an amicus brief on a petition, the Court granted only 5% of the cases.

We think that the relationship between the decision on a petition for certiorari and the filing of amicus briefs is much more complicated than the simple, bivariate association indicates. In particular, we expect that most organizations, ever mindful of the need to sustain the support of members, attempt to demonstrate the effectiveness of their activities in litigation and as a result are more likely to prepare and file briefs for those cases having a reasonable chance of acceptance. Previous research indicates that cases are most likely to be selected for plenary review when real conflict is present, when the ideological outcome in the lower court is at odds with the preferences of a majority of the justices, and when the solicitor general is the petitioner. Accordingly, we view them as important variables to control.

Our data, like those in other studies, demonstrate a substantial connection between a positive decision on certiorari on the one hand and the presence of the United States as a petitioner or of real conflict or ideological outcome in the lower court on the other. Of the 1,906 petitions for certiorari in our sample from the 1982 term, 109 (5.7%) involved cases with "real" conflict,[13] and of those 109, 76 (70%) were granted certiorari. For information on alleged and "real" conflicts in our sample, we have relied on a series of special issues of the *New York University Law Review* comprising the New York University Supreme Court Project (1984a, 1984b; see also Estreicher and Sexton 1986). The staff of the *New York University Law Review* made judgments for each of the 2,061 cases on whether the petitioner called attention to a conflict between or among courts and whether the alleged conflict was in fact a "square" or "real" conflict. A conflict, according to Estreicher and Sexton, "occurs when two or more courts of last resort—federal courts of appeals or state courts of last resort—take contrary positions on the same legal issue. We term this as a *square* conflict" (1986, 54). From everything we can discover, the staff of the New York University Supreme Court Project seems to have executed the research with extraordinary case and diligence.[14] For the purposes of this article, we have created two variables from that source: alleged conflict (whether the petitioner claimed conflict between two or more circuits, two or more state supreme courts, two or more federal or state courts, or the lower court and precedents of the Supreme Court), and "real" conflict (whether or not the staff of the *New York University Law Review* concluded that conflicts of the sort alleged were present). Further, we have coded the number of alleged and real conflicts present in each.[15]

In addition to the Court's propensity to review conflictual cases, we anticipate that justices are most likely to hear those cases they would like to reverse on ideological grounds. For each of the 1,906 cases in our sample, we have used *United States Law Week* and *Briefs and Records* to make a judgment about the ideological direction of the decision in the court below—that is, whether the lower court decided the case in a liberal, conservative, or an ideologically indeterminable or ideologically irrelevant fashion.[16] Clear liberal or conservative assessments were made in 1,773 of the 1,906 certiorari cases. The outcome at the lower court on these cases was predominantly conservative, with just under 40% being decided in a liberal direction. Yet these liberally decided cases were much more likely to be granted plenary review. Only 3.8% of the cases decided conservatively were granted certiorari whereas 16.1% of the liberal cases reached the plenary stage. And the presence of the U.S. government as petitioner also appears to be an excellent indicator of certiorari. Among the cases in our 1982 sample a writ of certiorari was granted in two-thirds of the cases petitioned by the United States.

Because amicus activity tends to coincide with factors that enhance a case's chances for plenary treatment, the independent effects of amicus activity can be identified only after carefully controlling these effects. We implement these controls in the following probability model. We specify the probability model that the Court grants a writ of certiorari on the *i*th case in the paid docket as

$$\Pr(\text{Cert}_i) = \beta_0 + \beta_1 x_{i1} + \ldots + \beta_{10} x_{i10} + e_i$$

where x_1 through x_{10} are coded respectively as follows:

1. $x_{i1} = 1$ if the federal government is petitioning party,
   0 otherwise
2. $x_{i2} = 1$ if the appellate court reversed a lower court decision,
   0 otherwise
3. $x_{i3} = 1$ if conflict was *alleged* by the petitioning attorney in one or more of the following situations:
   a. conflict between two state supreme courts
   b. conflicts between two federal circuit courts
   c. conflict with U.S. Supreme Court precedent
   d. conflict between a state court and a federal court,

   0 if no alleged conflict or more than one alleged conflict
4. $x_{i4} = 1$ if *real* or *square* conflict occurred in one or more of situations 3a–d,
   0 otherwise
5. $x_{i5} = 1$ if the case concerned civil liberties,
   0 otherwise
6. $x_{i6} = 1$ if the court's decision immediately below was ideologically conservative,
   0 if the lower court decided liberally
7. $x_{i7} = 1$ if one amicus curiae brief was filed in support of certiorari,
   0 if more than one brief or no briefs were filed
8. $x_{i8} = 1$ if two or three amicus briefs were filed in support of certiorari,
   0 otherwise
9. $x_{i9} = 1$ if four or more amicus curiae briefs were filed in *support* of certiorari,
   0 otherwise
10. $x_{i10} = 1$ if one or more amicus curiae briefs were filed in *opposition* to certiorari,
    0 otherwise

Table 1. Probit Coefficients for Certiorari Model

Variable	Coefficient	t-Statistic
Intercept	-2.14	-14.96
Solicitor general	2.16	8.62
Disagreement among lower courts	.29	2.26
Alleged conflicts	.28	3.58
Actual conflicts	1.44	10.49
Issue area	.09	.63
Ideological direction in court below	-.71	-5.16
One amicus curiae brief in support of certiorari	.81	3.94
Two or three amicus curiae briefs in support of certiorari	1.42	5.01
More than three amicus curiae briefs in support of certiorari	1.74	5.21
One or more amicus curiae briefs in opposition to certiorari	.36	2.02

Note: N = 1,771 (141 grants, 1,630 denials); -2 log likelihood ratio = 1970.1; pseudo R-squared = .80.

We amassed information for variables 1, 2, 5, and 7-10 from *Briefs and Records of the United States Supreme Court* on microfiche and drew the other variables from issues of the *New York University Law Review* and *United States Law Week, Law Week.*

Because of the nonlinear form of probability models, we estimated the coefficients in the model through a maximum likelihood probit procedure. The estimated coefficients and their t-statistics appear in Table 1. The number of cases in Table 1 is based on all 1,906 certiorari petitions in our 1982 sample but reflects the loss of 135 cases due to missing data. We excluded a total of 133 of the 135 cases because the ideological direction of the lower court's decision could not be ascertained.

Overall, the model as defined fits the data very well. Taken together, the coefficients for the independent variables provide a significant improvement in explanatory power over the intercept alone. The probability of a chi-squared value of 1,970.1 (minus twice the log likelihood ratio distributed as chi-square with 10 degrees of freedom) occurring by chance is virtually zero. Furthermore, the pseudo R-squared value, which takes on a value of one when the full model is a perfect predictor and a value of zero when the full model predicts no better than the intercept alone, is relatively large. Among the individual coefficients, only one does not attain statistical significance at a reasonable level of confidence (.05). The Court, it appears, is little inclined to hear one particular type of case over another—all else being equal.[17] This finding, of course, runs counter to the conventional wisdom.

The predictive accuracy of the model is also quite good in terms of distinguishing between the cases the Supreme Court will select for further review and those on which it will deny certiorari. Of course we need no statistical model at all to predict with great accuracy the percentage of cases that the justices will choose for plenary treatment. One need only predict that the Court will deny certiorari in all cases in order to be correct 92% of the time for our 1982 sample; the Supreme Court granted certiorari in 141 of the 1,771 cases in the probit analysis. Accordingly, we must assess the predictive accuracy of our model against this baseline of 92%.

In order to compare the model's predictions against this baseline, we chose an estimated probability of .31 as a cutoff for predicting whether or not the Supreme Court would grant certiorari. Cases with an estimated probability of certiorari being granted at or above .31 were predicted to be given full treatment, and denial was predicted for cases with probabilities below .31. We chose this cutoff because it yielded a prediction of a grant of certiorari in 141, the actual number of cases in the probit analysis found over for plenary treatment by the Supreme Court.

Of the 141 cases identified by the model as most likely to be accepted for plenary review, 93 (66%) were in fact granted certiorari. And of the 1,630 cases identified by the model as least likely to be granted certiorari, 1,581 (97%) were in fact not granted certiorari. Thus a total of 97 errors in prediction resulted from the full model, as compared to 141 without a model. Compared with other models of certiorari, we think a 31% reduction in prediction errors is excellent, although there is obviously much room for improvement.

Most importantly, however, our statistical results indicate strong support for our primary hypothesis. The coefficients for the three amicus curiae variables provide strong support for the proposition that justices pay close attention to the demands of outside parties when making certiorari decisions. Not only does one brief in favor of certiorari significantly improve the chances of a case being accepted, but two, three, and four briefs improve the chances even more. Given our efforts to control other important forces in the certiorari process, we are impressed with the striking significance of this result. The coefficient estimates for briefs in support of certiorari are three to five times larger than their standard errors for all three variables. Thus it is highly unlikely that these large effects are random occurrences.[18]

Most of the cases in our full sample where an amicus brief was filed prior to the certiorari decision had only one brief filed, but a noteworthy number of cases did attract multiple briefs. Single briefs were filed in support of certiorari in 71 cases, but two or three briefs were filed in 28 cases, and more than four briefs were filed in 20 different cases. The largest number of amicus briefs filed prior to certiorari in any one case was nine. In general, the more briefs filed in favor of certiorari on any given case, the better the chances for plenary review. It must be noted, however, that the marginal increment in the probability of certiorari being granted decreased with additional briefs.[19]

Importantly, briefs amicus curiae in *opposition* to a petition for certiorari significantly *increase*—rather than decrease—the likelihood that the Supreme Court will grant a review. This result is not nearly so striking as that for briefs in support of certiorari, but the chance of observing a coefficient as large or larger than that reported in Table 1 purely by accident is less than 1 in 20. Further, we note that the coefficient for briefs in opposition is larger than that for reversal between the lower courts. In any event, this result further supports our theory that an amicus brief at the agenda-setting stage is a signal by organized interests to the Supreme Court about the practical importance of a case. The direction of the statistical result suggests that what matters most is the presence or absence of an amicus brief per se, not the direction of the substantive arguments presented. If justices were strongly influenced by the contents of the briefs themselves, it is quite unlikely that briefs in opposition to certiorari would significantly increase the likelihood of certiorari being granted. Briefs amicus curiae in opposition, plainly and simply, pique the Court's interest in a case.

The amicus curiae briefs on our sample were filed by an extremely diverse collection of entities, including corporations, labor unions, professional and trade associations, ideological and single-issue membership groups, religious organizations, racial and ethnic groups, individuals, and units of local, state, and federal government (Caldeira and Wright 1988).

While we have not yet had the opportunity to analyze possible variations in the significance of briefs from all of these different types of organizations, we have isolated one important set of amicus briefs —those filed by the U.S. solicitor general. Because of the prestige and experience of the solicitor general's office, its involvement often carries considerable influence in the Court's certiorari decisions. As a result, we were concerned that the significant effects of amicus activity observed in the initial estimations might be due largely to briefs from the solicitor general's office.

Further analysis, however, indicated that this concern was unwarranted. In our sample of 1,906 cases, the solicitor general filed amicus briefs prior to certiorari in only 18 instances, not enough to exert a systematic and significant effect by itself on the Court's certiorari decisions during the 1982 term. Even with these cases controlled, the statistical impact of the remaining amicus briefs was essentially unchanged from the estimates reported in Table 1. Thus, even though we cannot yet say which groups or which types of groups have the greatest impact on certiorari decisions through amicus activity, we can state with a considerable degree of confidence that amicus curiae briefs from nonfederal entities have a strong and significant impact on the Court's propensity to grant certiorari.

Amicus activity, although a powerful predictor of certiorari, is one among several strong predictors in the model. One of the most important variables is the presence of actual conflict between circuit courts, between state supreme courts, between federal courts and state courts or conflict with Supreme Court precedent. Whenever actual conflict was present, the likelihood that certiorari was granted jumped dramatically. Given the previous research on the importance of conflict for setting the Court's agenda, a strong effect for conflict was expected but perhaps not quite as strong as found here.[20] More interestingly, though, *allegations* of conflict are also important for predicting certiorari, and the more allegations, the better the chances that the Court will accept a case. Evidently, the best advice to petitioning attorneys is to allege as many conflicts as possible. There is some evidence that attorneys understand this, for allegations of conflict were quite common during the 1982 term. At least one of the four types of conflict was alleged in nearly two-thirds of the cases considered for plenary review.

Closely related to conflict is the presence of a split decision, or reversal, between lower courts. In cases where a federal circuit court has overturned the decision of a federal district court, for example, the probability that certiorari will be granted is significantly higher. While not one of the strongest predictors in the model, a dispute between lower courts nevertheless is a factor that does distinguish a case and nontrivially increases its chances for further review.

The Court's attention to conflict and to disputes in the lower court underscores the justices' institutional concern with resolving conflicts of opinion on cases of wide importance. Yet, as we have hypothesized, justices appear also to have political agendas of their own, and these agendas are realized in part through the justices' ideological decisions about which cases to hear. Other things being equal, the Court is significantly more likely to hear cases that were decided liberally in the court immediately below. After Justice O'Connor's appointment in 1981, the Court had a decided, but certainly not extreme, conservative ideological orientation. Thus the clear tendency for the Court to select cases decided liberally in the court below affirms our theoretical expectation that justices' ideological predilections affect their decisions in much the same way that other elite political actors are motivated by their personal

Table 2. Estimated Probabilities of Certiorari for Selected Values of the Independent Variables

Solicitor General As Petitioner	Actual Conflict	Two or Three Amicus Briefs in Support of Certiorari	Estimated Probability
no	no	no	.01
no	no	yes	.35
no	yes	no	.33
no	yes	yes	.88
yes	no	no	.37
yes	no	yes	.96
yes	yes	no	.97
yes	yes	yes	—[a]

[a]The sample contained no actual cases with all three factors present.

ideological agendas.

It may well be that, as the motto of the solicitor general says, "The United States wins its point where justice is done its citizens in the courts"; but the fact that the Court's agenda is shaped by ideological considerations is keenly appreciated by the U.S. solicitor general's office, which itself has an ideological agenda to fulfill. Of the 56 cases during the 1982 term petitioned for review by the federal government, 73.4% were cases with liberal outcomes in the court immediately below. This ideological compatibility between the cases advocated by the federal government and selected by the Court suggests that the solicitor general should be relatively successful in convincing the Court to accept cases for full review, as indeed is the case. Of the parameters in our model, the size of the coefficient for the United States as a petitioner is second to none. The significance of a petition from the United States is even more impressive and interesting, given that much of the ideological basis of the Court's behavior has been controlled through the effect of the lower court's decision. Thus, ideological considerations aside, the solicitor general's expertise is evidently highly respected by the justices.

Having controlled for these various influences, we now ask, In probabilistic terms, what is the marginal impact of amicus curiae briefs on the Supreme Court's decision to grant or deny certiorari? In Table 2 we display the estimated probabilities for cases, given the presence or absence of three significant forces: the United States as a petitioner, actual conflict, and amicus briefs. We computed these estimated probabilities using the coefficients reported in Table 1, together with mean values for the remaining independent variables.[21]

The estimated probability that the Supreme Court will grant certiorari is .01 when none of the forces is present—that is, when the United States is not a petitioner, when no actual conflict is apparent, and when no one has filed an amicus brief in support of certiorari. Most of the cases in our 1982 sample were of this type. If we hold all characteristics of this hypothetical case constant except for the addition of two or three amicus curiae briefs, the estimated probability of acceptance increases to .35. Alternatively, for the same basic type of case with no amicus curiae briefs but at least one actual conflict present, the probability of a grant goes up to .33. The addition of two or three amicus curiae briefs to a case with an actual conflict raises the predicted probability of a grant of certiorari to .88. We predict similar changes as a result of the addition of two or three amicus briefs for a case in which the United States petitions for certiorari but which contains no actual conflict.

In general, then, amicus curiae briefs filed in support of the petition for certiorari increase the estimated probability that the Supreme Court will grant by a magnitude of .5 or .6, depending upon the characteristics of a particular case. It is true, of course, that the Court does not grant certiorari in all cases in which an organized interest happens to file an amicus brief in support. But for cases with a reasonable chance of acceptance in the first place, amicus curiae briefs can mark the difference between success and failure. For organized interests, according to our results in Table 2, the most cost-effective strategy is to file an amicus brief in cases in which *one* but not *both* of the two variables—United States as a petitioner and actual conflict—is present. When both are present, because the probability that the Supreme Court will grant certiorari is already at .96 or .97, the filing of an amicus brief can make little or no difference. Indeed, organized interests seem to recognize this point, for we found no cases in our 1982 sample in which amicus briefs, actual conflict, and the United States as a petitioner were all present.

Conclusions

We have proposed that U.S. Supreme Court justices attempt to select cases for plenary review with the greatest potential social, economic, or political significance consistent with their own ideological predispositions. We assumed that the potential significance of Supreme Court decision making is proportional to the demand for adjudication among interested parties and that justices acquire information about demand, or significance, by observing the extent of amicus curiae participation. Thus our primary hypothesis was that one or more amicus curiae briefs advocating or opposing certiorari would increase the likelihood that certiorari was granted.

The data we have collected from the 1982 term offer strong support for this hypothesis and, correspondingly, for our theoretical perspective on the role of interest groups in Supreme Court decision making. When a case involves real conflict or when the federal government is petitioner, the addition of just one amicus curiae brief in support of certiorari increases the likelihood of plenary review by 40%–50%. Without question, then, interested parties can have a significant and positive impact on the Court's agenda by participating as amici curiae prior to the Court's decision on certiorari or jurisdiction.

We believe this result is important both for understanding judicial behavior and for understanding the strategies and influence of organized interests in national politics. The U.S. Supreme Court, in our estimation, is quite responsive to the demands and preferences of organized interests when choosing its plenary docket. In this regard, the Supreme Court is very much a representative institution, and we think it both useful and important to analyze the Court from this perspective. We view the Court's openness to outside demands when choosing its plenary docket as not only necessary for a smoothly functioning representative polity but also as a natural consequence of rational political decision making. To be sure, how well or whether judicial responsiveness to the demands of organized interests comports with more traditional theories of decision making is a matter of considerable controversy and is best left for fuller treatment elsewhere.

Organized interests are generally influential during the Court's agenda phase because they solve an informational problem for the justices. Through participation as amici, organized interests effectively communicate to the justices information about the array of forces at play in the litigation, who is at risk, and the number and variety of parties regarding the litigation as significant. Obtained elsewhere, this information would surely be much more costly than as provided through amicus participation, if it could be obtained elsewhere at all. In this regard, organized interests function effectively as interest articulators, a result that generally adds stability to political systems.

In the near future we foresee several extensions of the present analysis. First, we are in the process of gathering information on sponsorship for the cases in our 1982 sample. We think sponsorship holds considerable promise as a predictor of case selection. Second, we are in the process of replicating, as much as possible, all of our variables for the 1968 term. It appears that the scope and intensity of interest group participation has changed considerably since that time, and we think it important and useful to document and explain this change and its consequences. Third, we intend to test the model presented, as well as further refinements of it, on the votes of the individual justices. For example, do some justices respond with greater favor to amicus briefs filed prior to the decision on certiorari? Do some evince greater sensibility to the presence of conflict between or among courts? Finally, we are preparing to survey attorneys for, and representatives of, organized groups in order to understand better the strategies and perceived costs and benefits associated with the role of amicus curiae, as well as other forms of participation in litigation. If amicus participation is as important as our statistical analysis suggests, then we should find considerable corroborating evidence from the attorneys with first-hand experience of how the Court operates.

Notes

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1. We use the terms *organized interest, interest groups, organizations,* and *groups* more or less interchangeably, having in mind a very broad conception. It includes under the umbrella "not only associations such as trade associations, unions, professional associations, and environmental groups that have individuals or organizations as members, but also politically active organizations such as universities, hospitals, public interest law firms, and especially, corporations that have no members in the ordinary sense" (Schlozman and Tierney 1986, 10). We employ *organized interest* to denote "the wide variety of organizations that seek joint ends through *political* action" (Schlozman and Tierney 1986, 11). With rare exceptions, *organized interests,* not individuals, participate as amici curiae.

2. In light of the extraordinary amount of labor entailed in gathering the data needed to test our model, it is simply unrealistic to collect data for more than one or two terms of the Court. We have no reason to believe that the 1982 term was peculiar in any respect. For an explanation of why we chose the 1982 term, see n. 11.

3. We plan, in the future, to investigate the impact of "sponsorship" and direction participation by organized interests on the Court's decision to grant or deny certiorari. Fragmentary evidence suggests the importance of this strategy at the gatekeeping stage (see, e.g., Sorauf 1976).

4. It is quite likely, given our theoretical model, that briefs amicus curiae will exert a greater impact at the gatekeeping stage than on the merits. But of course we do not test the differences between the two phases of decision here. For some evidence of the effects of amicus briefs on the merits, see Ivers and O'Connor 1987.

5. We recognize as well that a justice may not always vote to review a case even when he or she could muster four votes for certiorari (Brenner 1979). This is true because even though a majority decision on the merits requires the agreement of five justices, only four justices need consent to grant a writ of certiorari or to note probable jurisdiction on an appeal. So if a justice believes that a fifth favorable vote on certiorari or appeal is unlikely on the merits, then bringing the case forward for review is,

obviously, counterproductive. Linzer illustrates our point well: "There are surely . . . cases in which a Justice votes to deny certiorari despite his strong dislike of the decision below because he is afraid that there is a majority against him that would affirm on the merits. . . . A former Supreme Court clerk has privately retold how his Justice 'taught a lesson' to a new colleague. The experienced Justice had voted to deny certiorari in a case that both he and the new man found dreadfully decided below, despite three votes to grant. The new man sent a note pointing out that with the elder Justice's vote there would be the four votes needed for review, to which the elder asked back: 'But where will you get the fifth?'" (Linzer 1979, 1303, n. 539).

6. Of course we do not mean to imply that the quality of the legal arguments made in a brief is irrelevant.

7. We base this statement on our sampling of docket books and memoranda in the Papers of Mr. Justice Brennan and Chief Justice Earl Warren, Manuscript Division, Library of Congress. Nowadays, the justices rely heavily on law clerks to do the screening of petitions for certiorari and jurisdictional statements. During the period for which we present data, Chief Justice Burger and Justices Powell, O'Connor, Blackmun, White, and Rehnquist participated in the so-called "pool" for certiorari. Justices Brennan, Marshall, and Stevens choose to screen cases within their individual offices. The reliance on the help of clerks does not at all imply that the justices do not look for cues or signals in the cases. For the clerks, in preparing memoranda on petitions and statements, clearly attempt to ferret out telltale signs for their bosses (see, e.g., Provine 1980, 26). Indeed, the thumbnail sketches the law clerks provide probably reinforce the tendency to scan for a handful of facets about a case.

8. More recently, in *Anderson* v. *Ohio*, Justice Brennan's clerk wrote to him about a request to expedite consideration of the petition for certiorari. First of all, Brennan's clerk noted the presence of three amicus briefs for the states of Maryland, Maine, and New Mexico. He then went on to write about the situations as described in the briefs amicus curiae: "Ohio is not the only state whose early filing laws have been struck down. The laws in Maine, Maryland, and New Mexico have also been ruled unconstitutional. The decisions in Maryland, Kentucky, and Maine have been appealed. The First and Fourth Circuits have apparently established schedules and may already have heard their cases. The Sixth Circuit had not heard this case as of a week ago" (no. 80-182).

9. For example, in *Kramer* v. *Union Free School District No. 15 et al.*, no. 258, 1968 term, one of Chief Justice Warren's clerks wrote his memorandum around a dissenting opinion by Judge Weinstein in a three-judge district court. The issue there was whether the equal protection clause prohibited New York's restriction of the right to vote in school district elections to those who own or lease property or who have children attending the school. The clerk devoted less than a page to a discussion of the majority's view and reasoning and over two pages to Judge Weinstein's dissent. He concluded: "I think appellant has presented a close question on the merits which should be considered by this [Court]. Judge Weinstein's opinion makes a strong case for appellant's position and, more significantly, for the importance of the issues he has raised" (PJM to Earl Warren, 12 September 1968).

10. Scholars are maddeningly inconsistent about which issues might interest the Court more and how to define the various categories.

11. In practice this means that we have some cases with 1981 docket numbers ($N = 387$), but most of course have 1982 numbers ($N = 1{,}674$). At the end of each term, the Supreme Court carries over several hundred cases. Some of these await a grant or denial of certiorari, others oral argument and a decision on the merits. Regardless of which term one chooses, the sample will include a mixture of cases originally filed in different years. In selecting our sample of cases, we had three primary considerations in mind. First of all, we wished to collect data for cases in which the New York University Supreme Court Project had made assessments about the presence or absence of alleged and actual conflicts between or among courts. To execute a study of these conflicts would have required resources far beyond our command. Second, we wanted to pull together information for one full term of the Court. Third, the term chosen had to be recent, but not too recent. We plan to use our list of amicus participants to generate a partial list of organizations for a later survey, so a more recent term helps to maximize the rate of response. But since we rely on the *Briefs and Records of the United States Supreme Court*, which runs two or three years behind the *United States Reports*, we could not choose a term as recent as we otherwise might.

In *Redefining the Supreme Court's Role* (1986), Estreicher and Sexton tell of the New York University Project's sample: "It examines all of the cases that the Supreme Court agreed to hear (201) and all of the 'paid' cases that the Court refused to hear (1,860) during the 1982 Term. . . . We chose to study the cases the Court decided to hear during the Term, rather than the cases it actually heard, because we wanted to replicate as nearly as possible the case selection process that occurs during a particular Term" (p. 76).

12. We restrict the analysis to petitions for certiorari because, as a formal matter, the Supreme Court treats appeals somewhat differently. We did a separate probit for jurisdictional statements and found that the Court indeed makes its decisions on

appeals in a manner significantly different from those on certiorari.

13. This number is very close to Feeney's (1975) reported average of 119 cases with conflict during the 1971 and 1972 terms, and to Ulmer's (1984) reported average of 113 cases of conflict.

14. Here is more detailed description of the manner in which the Supreme Court Project conducted its business (Estreicher and Sexton 1986): "Using the criteria derived from our view of the Supreme Court's role, we have examined each of the granted cases. . . . We examined the 201 granted cases ourselves. Each one is discussed in the project as reported in the *NYU Law Review*. Twenty-two *Law Review* editors and four research assistants examined the 1,860 cases denied review. The students determined whether the denied cases presented an intercourt conflict of clash with Supreme Court precedent. . . . Our position on each of the granted cases and the students' position on each of the denied cases—2,061 in all—are summarized either in a textual discussion or in an appendix entry in that journal" (p. 78).

15. We have considered the effects of several other variables, some of them mentioned in previous literature: dissent in the court below and the allegation of a constitutional issue. Neither of these variables exerts a significant impact on the Supreme Court's decision to grant or deny a writ of certiorari.

16. Anyone who has ever attempted to classify judicial decisions according to the ideological direction of the outcome will readily attest to the difficulty of this task. In many—perhaps most—of the cases, we found the ideological direction of the outcome clear and easily coded. In a certain percentage of the cases, we simply could not make a classification. For the most part we have opted for a fairly conventional coding procedure along the lines Spaeth, Goldman, and others have articulated over the years. Some have argued that scholars should code the outcome of a case according to what kinds of parties win or lose, for example, for or against labor unions or criminal defendants. We tried to blend together the best of both approaches.

17. We recast issue area in several different forms. For instance, we included dummies for criminal, civil liberties, and economics cases; and none was significant. Regardless of how we coded issue area, we turned up nothing of significance.

18. Naturally, organizations file briefs amicus curiae for many of the same reasons as the Supreme Court grants petitions for certiorari. One might, therefore, suspect simultaneity in our equation. But the variables in our model reflect the quality of a case as a legal vehicle, not the social, political, or economic importance of the issue to an organization. In our theoretical model, briefs indicate a decision on the part of an organization to signal to the Supreme Court the importance to it of a case. To test for the possibility that the filing of briefs amicus curiae on certiorari is a function of the same variables as is the decision to grant or deny, we regressed the number of briefs filed on a case on all of the independent variables in the model presented in Table 1. The results differ in important respects from those reported in Table 1. Interestingly, although the presence of the solicitor general as a petitioner exercises an important influence on the decision to grant or deny, it does *not* exert an impact on whether an organization filed a brief amicus curiae. We believe, in sum, that the decision to file an amicus curiae is independent of the decision to grant or deny certiorari.

19. We entered amicus activity into the model in three different ways, in addition to the results reported in Table 1. One specification included a single dummy variable indicating the presence of one or more briefs or no briefs. A second specification included just the actual number of briefs filed, and the third specification included the total number of amici listed on all briefs for each case. All of these treatments yielded highly significant effects for amicus activity. We finally settled on the specification reported here because it illustrates both the added value of more briefs and the decreasing marginal increments in probability of additional briefs.

20. There are several possibilities for why our results for conflict appear stronger than those reported by Ulmer. First, and most obviously, the procedure for identifying square conflict in the variable we use is different from his procedure. And second, whereas Ulmer used discriminant analysis, we have relied on probit analysis. For a comparison of the two techniques, see Maddala 1983.

21. To compute each of the probabilities reported in Table 2, we used different means for the ideological outcome in the lower court, disagreement among lower courts, issue area, and amicus briefs in opposition. We computed each mean on a different subsample of cases corresponding to a particular combination of actual conflict, the United States as a petitioner, and amicus briefs.

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ORGANIZATIONAL MAINTENANCE AND THE RETENTION DECISION IN GROUPS

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Understanding why members leave or remain in groups has received little attention despite its fundamental importance for organizational maintenance. In this analysis, a theory of experiential search is proposed and applied to Common Cause. Group participation is conceptualized as a process by which imperfectly informed decision makers learn about the organizations they join. This framework makes quitting understandable and provides a link between the initial membership choice and follow-up decisions.

Organizational maintenance is a fact of life all group leaders confront. For the majority of interest group entrepreneurs, who depend on constituent dues as a prime funding source, maintenance dictates the need to keep members contributing (but see Walker 1983).[1] Even seemingly small drops in numbers—10% or 20% net of replacements—are viewed with great alarm; and the loss of long-time contributors is perceived as a threat to the entity's survival. The key to creating a successful organization can be summarized simply: entice potential members to join, keep attrition below the rate at which replacements can be found, and establish a core membership.

If this prescription is correct, it is curious that analysts of interest group membership focus almost exclusively on first-time joiners.[2] Most authors do not deal explicitly with the dynamics underlying the *retention* choice, that is, the decision by existing members to remain in the group and keep on contributing.[3] Attracting members is fundamental for long-term organizational prosperity, but signing them up in the first place is only half the battle. The conditional joining decisions on whether to stay in the organization are also crucial.

Interest group entrepreneurs encounter a dilemma: how to retain members for whom leaving may be an attractive option.[4] The leaders focus on producing the selective incentives that the membership wants. They are also preoccupied with not antagonizing constituents, for fear they will cease contributing. They structure and operate the interest group so as to facilitate contributions (Moe 1980). How else, for example, can one explain the elaborate lengths to which many leaders will go to ensure the appearance of rank-and-file participation in the organizational decision-making process?

In the political arena, the retention problem should hit home hardest for public interest groups. Their leaders lack the occupational or industry bases that underlie so many private associations. They cannot draw on a "natural" membership, among whom either selective incentives are easily generated or coercion induces contributions. That people sign

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up for public interest groups in the first place in light of the collective action dilemma—not to speak of retaining their organizational allegiance—is a phenomenon that has generated considerable scholarly interest (Berry 1977; Smith 1985).

Why, then, do members of an organization, especially a public interest group, choose to remain? Is this process indistinguishable from the original decision to join—whatever that may be? How are the two connnected, if at all?

These issues will be explored by analyzing what is popularly considered the quintessential public interest group: Common Cause. In the fall of 1981, over 12 hundred Common Cause members were surveyed using a mail questionnaire. All respondents were queried about a wide variety of issues—membership and its benefits, personal attitudes, previous history in Common Cause, and future intentions, to name a few. A stratified design oversampled Common Cause-designated activists, who were defined as steering committee coordinators in congressional districts or coordinators or activators of telephone networks. They comprise 23% of the sample, a roughly sixfold overrepresentation. Since this stratification is based on characteristics that are exogenous to the retention decision, this sampling strategy has no impact on the hypothesis tests conducted in this analysis (Amemiya 1985; Hausman and Wise 1979). These data furnish a rare opportunity to explore the retention decision and to learn why people participate in political organizations despite all the obstacles to collective action.[5]

The first part of this analysis lays out the theoretical groundwork by specifying alternative conceptualizations of the membership renewal process.[6] Despite frequent assertions in the literature that the study of organizational membership is theory-rich and data-poor (e.g., Arnold 1982; Shaiko 1986), it is argued here that the available models require further development. Once the proper theoretical underpinnings have been laid, the focus shifts to the empirical world. Unlike prior descriptive work on the original membership decision by authors who fail to test hypotheses, competing retention models are made operational and tested to determine whether it is possible to make sense of members' decisions to stay in or leave organizations.

Theoretical Perspectives: Joining, Remaining, and Experiential Search

Although this study looks primarily at renewal decisions, the initial reason for joining is the logical place to start. In the first place, virtually all of the available theoretical research focuses on the original membership calculus. Second, an analysis of the initial choice furnishes a vantage point from which to consider the renewal decision, and vice versa. The challenge is to integrate perspectives on both initial contributions and retention decisions.

Parenthetically, the viewpoint taken in this analysis and the tradition of research in which it is written is decision-theoretic rather than game-theoretic, despite the latter's importance for studying collective action (e.g., Axelrod 1984; Hardin 1982). While game theory furnishes many insights into how actors behave strategically in small-number situations, it is inappropriate for explaining why individuals contribute—and keep contributing—to large groups. Decision-theoretic models in which individuals are playing against nature are more suitable for understanding the retention choice.

Most theorists working in this research tradition (e.g., Clark and Wilson 1961; Moe 1980; Wilson 1973) typically identify a number of benefit types: selective, solidary, and purposive. *Selective* benefits are tangible returns that have monetary values and are derived from contributions. They either may be divisible, private rewards or may emanate from members' impacts on the level of collective goods provided to everyone. *Solidary* benefits stem from associational interactions, while *purposive* benefits are intangible rewards garnered from contributing to the group because of its stated goals.[7] These theorists then examine which of these benefit types are important and why.

In the mid-1960s, Mancur Olson (1965) revolutionized perceptions about why citizens join organized groups. His seminal contribution details the difficulties associated with collective action. Individuals join large groups, he argues, because the value of the available selective benefits exceeds the costs of membership.

Olson assumes that individuals have full information, are only interested in economic rewards, and maximize without error. In large groups, this implies that only dues levels and divisible selective benefits will be relevant. Political activity is simply a by-product of narrowly self-interested behavior. Given this framework, it is obvious that the conditional membership decision should follow the same cost-benefit analysis. Since in general not much will change from one contribution period to the next, particularly given the assumption of perfect information, little explicable organizational attrition is possible.

Those challenging Olson's theory have taken two principal tacts. Some (e.g., Clark and Wilson 1961; Moe 1980; Wilson 1973) emphasize that purposive and solidary incentives are important, along with selective returns, in the decision calculus.

Individuals who possess perfect information but are not satisfied by selective incentives alone may join because the potential purposive and solidary payoffs push them over the threshold where benefits exceed costs. People derive consumption benefits from interpersonal interactions and the purposive statements their contributions make. The multiplicity of incentives can be easily incorporated into the Olsonian framework, and the same inferences about the conditional membership decision still hold. This argument is intuitively reasonable; Olson himself recognizes the potential importance of nonmonetary returns but ignores them for reasons of parsimony.

Another critique is that the key problem lies less with the breadth of incentives than with the assumption of perfect information. Moe (1980) maintains that potential group members do not possess perfect information. Consequently, some decision makers miscalculate their own contributions to the provision of collective goods and hence the level of selective incentives the group offers. A subset of the population mistakenly thinks of itself as highly influential and incorporates its allegedly substantial contributions to the provision of collective goods into its membership calculi. Individuals whose perceptions of their efficacy put them at the upper end of the population distribution join in disproportionate numbers. The assumption seems to be that once these contributors miscalculate, they keep repeating the same mistake over and over. Again, organizational membership ought to be stable, and there should be little explicable attrition.

The relaxation of the perfect information assumption is eminently reasonable and extremely important. But precisely how imperfect information affects contributions needs to be rethought. It ought to influence more than simply individuals' initial estimates of the benefits they derive from contributing to collective goods.[8] The conclusion that many members join organizations only because they make mistakes is unsettling—especially because educated individuals tend to join in greater proportions. Implying that those committing errors keep contributing for years without revising their prior beliefs is even more difficult to believe. As in the two models elaborated above, the conditional membership decision replicates the initial choice to join. While there may be some exogenous forces prompting a few individuals to leave, the outcomes generally ought to be the same under all three models.

A more reasonable supposition is that the decision to join makes sense as a strategy by individuals who recognize their lack of knowledge. Members join groups to learn about them, and as they acquire knowledge, some can be expected to leave. The politics of *experiential search* offer a superior perspective for understanding retention and integrating it with the initial membership choice.

A Theory of Experiential Search

Organizational membership can be conceptualized as a search process. Citizens lack complete information about all of the alternative groups they might join and the associated costs and benefits. They presumably would like to discover an organization or organizations that will give them enough returns relative to costs—regardless of the types of benefits they seek—that they will be content to remain.

Experiential search can be distinguished from the economics literature on sequential search (for an excellent recent overview, see Mortensen 1986). In the latter, individuals typically search sequentially until they meet their reservation wage or price, where the expected marginal cost of an additional iteration equals the expected marginal return. In these models, workers or consumers gather information before taking a job or purchasing a product.

There are some models (e.g., Burdett 1978; Wilde 1979) that permit on-the-job or experiential learning by workers within a sequential search framework. These perspectives are much closer in spirit to the experiential search theory propounded here, but there are still important differences. For example, by and large, workers *must* search for a job, while there is no similar compulsion to join a public interest group.

In looking for an organization to fill their needs, individual decision makers with fixed preferences have three options: they can (1) conclude that given the problems of obtaining information and the costs of membership, it is advisable to give up; (2) search over alternative associations without contributing; or (3) join an organization to learn whether membership is worthwhile. This final option, learning through exposure, can have a number of facets—developing an understanding about how a group functions, whether it is effective in achieving its goals, and whether its outputs are in line with one's preferences, to name a few. Given the low monetary cost of joining numerous voluntary associations, particularly public interest groups, many should opt for experiential learning because it is an efficient information-gathering technique.[9]

A factor predisposing searchers to join an organization is that many attributes of membership are only observable by participation. These are specific characteristics, while those that can be observed without joining are general characteristics. These two types can be thought of as opposite poles on a continuum that reflects the difficulty of acquiring information without making a commitment; most informational traits combine elements of both.

Almost every factor incorporated into the joining (or retention) calculus can be described, at least partially, as a specific characteristic. The only purely general feature of organizational membership is the dues level. If the remaining factors were ranked from more to less general, they might roughly be ranked as follows: costs other than dues levels, purposive benefits, divisible benefits, solidary benefits, and collective benefits.

When the costs of evaluating specific qualities are relatively low, prospective contributors will tend, ceteris paribus, to join, accumulate knowledge, and then decide either to quit or to stay and learn more. Since a reasonable inference is that one accumulates knowledge more and more slowly over time, the expected rate of dropping out should diminish temporally. Specific characteristics should also be more important for newcomers than for veterans.

In the organizational context, first-time joiners will have imperfect information about costs other than dues. How can the costs of phone calls asking for assistance—writing legislators, contacting other members, and so on—or appeals for monetary contributions in excess of dues be established without error in advance? As contributors spend time in the group, they will develop a growing awareness of the true price of membership and behave accordingly.

Along these same lines, the value of purposive benefits will be increasingly evident with experience. Although members will probably have some initial idea about what the group stands for, they will gradually learn whether it represents those things that provide them with consumption benefits.

Before they join, potential members will also lack complete information about the quality of the divisible benefits furnished to participants. Accurately assessing the full value of a groups' offerings without consuming them regularly is all but impossible; membership offers an opportunity to learn about their utility firsthand.

This inference has an important implication: Suppose that before engaging in experiential search, small contributors deduce that their donations will have no impact on the level of collective goods provided. Presume too that purposive and solidary incentives play no role in eliciting contributions. Even in this extreme case where lack of knowledge is irrelevant for an individual's valuation of collective, purposive, or solidary benefits, imperfect information *still* ought to be a major factor in members' decision calculi.

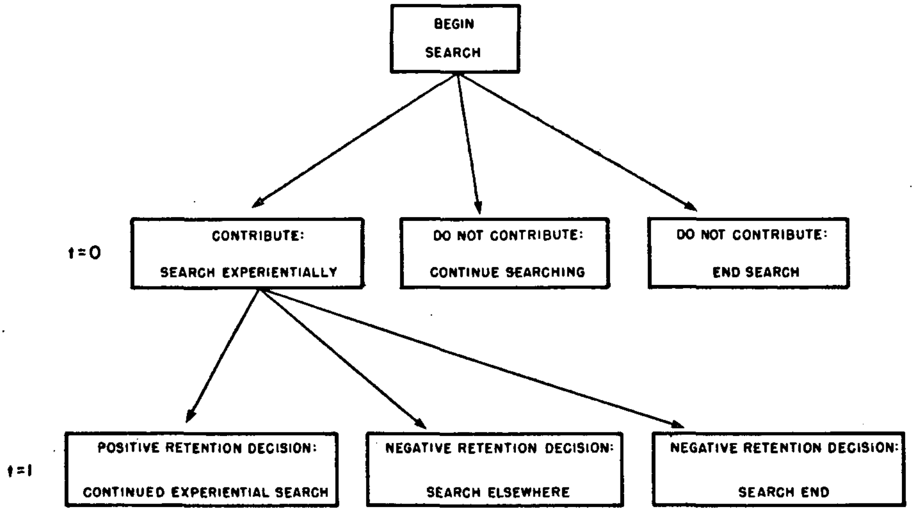
If contributors are motivated by solidary returns, it is unlikely that they will have a full idea of the value of these benefits either. Only immersion in day-to-day organizational operations will permit them to see whether the interpersonal interactions are sufficiently rewarding.

Experiential search should be especially germane for learning about collective goods. Member education should have several elements. Contributors ought to become more cognizant of what collective benefits their organization actually proffers. Furthermore, those who believe that their contributions to the provision of collective goods are nontrivial should learn about the value of these goods and the insignificance of their contributions. Members who donate resources to have an impact on the magnitude of public goods provided—and not because they gain purposive rewards from contributing to collective goods—should leave the group over time, ceteris paribus.

To reiterate, citizens with perfect information (or those who have imperfect information but never learn) will join those organizations that have the highest net benefits for them. While changes in exogenous conditions might lead some to exit, the strong presumption is, Once a participant, always a participant.[10] But when the assumption of imperfect information is embedded within the experiential search framework, quitting becomes comprehensible.

Group membership might usefully be conceptualized as a decision-theoretic process in which members join an organi-

Figure 1. Organizational Membership as Experiential Search


BEGIN SEARCH
t=0
CONTRIBUTE: SEARCH EXPERIENTIALLY
DO NOT CONTRIBUTE: CONTINUE SEARCHING
DO NOT CONTRIBUTE: END SEARCH
t=1
POSITIVE RETENTION DECISION: CONTINUED EXPERIENTIAL SEARCH
NEGATIVE RETENTION DECISION: SEARCH ELSEWHERE
NEGATIVE RETENTION DECISION: SEARCH END

zation and then reevaluate their decision in accordance with what they have learned about the costs and benefits of participating (see Figure 1). Each membership renewal period is another stage in this decision process. And each time, the contributor has better information. Learning about specific characteristics continues indefinitely, but the amount of additional information accumulated through experiential search diminishes over time. Members weed themselves out, especially during their first few years in an organization. Information updating can reinforce a propensity to remain in the group for another contribution period or lead to the conclusion that membership is less valuable than foregone opportunities. Those abdicating membership either become politically inactive or continue searching for an alternative that makes participation worthwhile. For those who stay in the organization, over-time learning should become a less and less salient factor in the decision whether to stay or go.

Experiential Search Theory: An Empirical Test

The experiential search theory implies that withdrawal is a rational response by imperfectly informed decision makers. Unlike the previous three models, under this formulation members' cost-benefit calculations should change substantially over time. This expectation has three implications that can be tested with cross-sectional data on Common Cause membership:[11] (1) a model of the conditional membership decision should uncover fairly strong relationships between costs and benefits, even for those individuals who have previously elected to join; (2) the longer contributors have been in the organization, the less likely they ought to be to leave, because the likely increment in knowledge declines over time; and (3) the impact of those factors that guide the retention calculus, particularly highly specific characteristics, ought to be stronger for relative newcomers.

The true test of this framework is how

well these theoretical assertions explain behavior in the empirical world. Too often students of organizational membership have been content to utilize a few descriptive tables—largely because they were analyzing the initial membership decision with data only about members. This research focuses on testable hypotheses about membership dynamics.

The empirical analysis of Common Cause renewal decisions unfolds in four steps. First, to provide the proper context, I outline the costs and benefits of contributing to the association. Second, I provide descriptive data about the reasons members say they initially joined, to establish a baseline for comparison with their subsequent decisions. Third, I examine whether those who belong to an organization learn over time, to provide side evidence about the assertions made earlier. Finally, I make models of the conditional membership process operational and directly test them to determine which is most empirically valid.

The Costs and Benefits of Common Cause Membership

Common Cause is the most prominent of the new wave of organizations associated with the burgeoning consumer movement of the late 1960s and early 1970s (for a detailed case study of Common Cause, see McFarland 1984). This feature alone makes it a natural candidate for study. But it is the leadership's strong dependence on its more than 200 thousand members that makes it especially appealing for analysis. The members are essential because of both their financial contributions and their activism at key points during the organization's external political battles. What, then, are the costs and benefits that motivate citizens to contribute to Common Cause?

Initially joining the association is a reasonably low-cost activity. Organizational dues are predictably moderate: $20 per year. Members are fairly well-off—their median family income in 1981 fell in the $25 to $35 thousand range, about 50% higher than the national average.[12] A tenable assumption is that relatively well-heeled contributors can learn about Common Cause or other moderately priced organizations through experiential search.

These members should discover new information about the value of belonging. They learn that there are additional costs to participation. Members are solicited to give more financial support, and many comply. In fact, 63% of those surveyed reported contributing more than the required $20. Despite this generosity, Common Cause, with a staff of less than one hundred, lacks the financial resources to compete on an even footing with private interest groups. Like public interest associations generally, it employs other instruments to make up for these shortfalls. One critical resource is energizing contributors to become politically involved. Members are asked to give of their time and become part of Common Cause's activist core. They are encouraged to immerse themselves in the association's sophisticated, congressionally based, grassroots network, as well as in state and local organizations.

It is difficult to be simply a passive contributor. Those preferring only to write checks will find themselves the targets of periodic mobilization efforts when battles over issues pinpointed by the leadership come to a head. Forty-three percent of all members in the survey (32% of the rank and file and 68% of the activists) said they had been contacted by Common Cause in the past year and asked to write to Congress about some issue.[13]

Contributors also have opportunities to learn about the complete gamut of organizational benefits. Although the stereotypical individual who joins Common Cause is characterized as a liberal do-gooder, the leadership obviously believes that divisible benefits are necessary to keep members happy. A bimonthly magazine is distributed to association members as an enticement to contribute; perhaps the consumption of this and the other political information furnished is sufficient to elicit a minimal annual contribution.[14]

Some may perceive another private return from participation, namely, career advancement. Suggesting self-promotion as a motivation is antithetical to the high moral ground that Common Cause tries to occupy, but it is consistent with Olson's by-product theory. An unexpectedly high proportion of respondents—23%—replied that they had political aspirations. Thirty-five percent of those surveyed stated that they either had political aspirations or had at some time sought a party position, an elected office, or an appointed office. When broken down between rank and file and activists, the percentages are 32 and 45, respectively. This discovery suggests that the participation of many allegedly altruistic liberals may be motivated by the search for a springboard onto the political opportunity structure (Schlesinger 1966) or perhaps by a desire to be educated about liberal positions. If it does the trick, membership is renewed; if not, the aspirant moves on and continues to search.

Common Cause has been involved in many celebrated battles, generally associated with "good government" issues designed to provide a host of nonexclusive collective goods. The leadership selects its policy areas carefully to maintain membership loyalty. To gather information about constituent preferences, the staff annually polls contributors for their opinions about the Common Cause political agenda. Choosing those issues over which opinions are particularly homogeneous is considered especially important (McFarland 1984).

This decision-making strategy has several interesting implications. It implies a belief that members either think they have an impact on the provision of collective goods or get considerable value from the purposive statement that their participation in Common Cause makes to the world. Assuming that the leadership's inference is correct, this conflict-minimizing strategy should also mitigate the impact on retention decisions of members' satisfaction or dissatisfaction over the association's political actions. If those in charge selected issues exclusively on other criteria, for instance, if they mistakenly viewed political action as a by-product that has no weight in participants' decision calculi, then contributors' evaluations of Common Cause's political efforts would be more germane in conditional membership decisions.

Common Cause's decentralized structure obviously provides ample opportunities for interpersonal interactions (see also McFarland 1984). Although much of this is a product of organizational weakness—lack of funds forces the staff to rely heavily upon its other major resource, an energetic membership—it may also be a strength when it comes to keeping contributors in the organization.

Why Join in the First Place?

Information on members' professed reasons for joining Common Cause cannot explain why people belong. There are no data on those electing not to sign up. However, contributors' beliefs about why they originally joined do furnish a vantage point from which the retention process can be understood. Are participants' reasons for staying or leaving consistent with their assessments of why they initially contributed? Or do other unrelated factors enter their calculations?

Using an open-ended format, members were asked why they joined. Their responses were coded according to the type of benefits they seemed to value most. The most intriguing result is that the vast majority of contributors believe that they got involved for purposive reasons (Table 1). Even employing an extraordinarily broad definition of selective benefits, 72% of respondents must be classified as professing that purposive concerns stimulated them to sign up.[15] These data are consistent with the experiential search argument that members know little about the specific costs and benefits of belonging when they initially join. General reformist tendencies were

Table 1. Members' Proclaimed Reasons for Joining Common Cause

Reason for Joining	Number of Respondents	Percentage
Purposive benefits		
Supports general goals, issues, or efforts	169	14.7
Keeps government honest and fair	162	14.1
Reforms, improves government; makes government more responsive	132	11.5
Combats special interests, lobbyists, PACs, corporations, big business	79	6.9
Supports public interest and the common good	71	6.2
Is a watchdog-investigatory group	53	4.6
Is a nonpartisan group	26	2.3
Protests political power and corruption	21	1.8
Supports in general (no issue content)	20	1.7
Is concerned about democracy	18	1.6
Is antigovernment or anti-political parties	17	1.5
Appeals to sense of civic duty	13	1.1
Addresses problems in society, government policies	11	1.0
Maintains checks and balances	10	0.9
Is unique and important	9	0.8
Needs financial support	8	0.7
Is a liberal and anticonservative group	6	0.5
Subtotal	825	71.9
Selective benefits (collective or divisible)		
Makes member effective	65	5.7
Provides political information	53	4.6
Addresses specific issue(s)	35	3.0
Offers a chance for personal political activity	32	2.8
Provides collective action necessary for change	28	2.4
Supplies leadership	26	2.3
Provides representation	18	1.6
Informs citizens and encourages participatory democracy	12	1.0
Offers a chance to be mobilized on issues	11	1.0
Is effective	9	0.8
Is an activist organization	5	0.4
Subtotal	294	25.6
Solidary benefits		
Family or friends belong	11	1.0
Joining is part of life style—social reasons	3	0.3
Subtotal	14	1.3
Miscellaneous	16	1.4
Total	1,149	100.2

Table 2. Organizational Experience and Knowledge about Group (%)

Years in Group	Percentage Correct 0	25	50	75	100	Mean Percentage Correct
1	34.1	31.7	26.8	4.9	2.4	27.5
2	34.9	23.3	30.2	8.1	3.5	30.5
3	20.4	18.4	28.6	25.5	7.1	45.2
4	9.6	19.3	22.9	42.2	6.0	53.9
5	7.3	15.9	35.4	30.5	11.0	55.5
6	12.1	13.6	28.8	30.3	15.2	55.7
7	5.4	14.7	28.7	31.0	20.2	61.4
8	2.5	13.6	25.9	42.0	16.0	63.9
9	4.9	10.7	29.1	32.0	23.3	64.6
10	3.4	11.2	16.9	47.2	21.3	70.0
11	2.8	8.4	25.2	28.0	35.5	71.3
12	5.3	5.3	23.7	39.5	26.3	69.1
Number of Cases	116	159	307	368	205	1,115

Note: $X^2 = 254$, $df = 44$. Each cell gives the percentage of members who have been in group for x years (row) who get the designated percentage (column) of the answers correct.

especially evident among those with a purposive impetus. Even many whose initial participation allegedly reflects the quest for selective benefits exhibit a broad participatory impulse. They claim they chose Common Cause to be politically effective or mobilized and generally not because of specific issues or other detailed benefits.

Do Members Learn?

An assumption underpinning the experiential search framework is that members learn about the costs and benefits of participating. More extensive knowledge about these organizational features can lead to higher or lower levels of retention. But regardless of whether members stay or leave, learning is a fundamental component of experiential search.[16] A prerequisite for validating this perspective is ascertaining whether contributors learn through organizational exposure.[17] In Common Cause, individuals with many years of associational experience are no different sociodemographically than newcomers. They are indistinguishable in terms of education or income, so any variation must emanate from other sources. Findings that long-term contributors know more about the organization would demonstrate that members learn and would provide important side evidence that experiential search is fundamental for associational membership.

Several straightforward tests clearly show that new arrivals and long-time contributors are only distinguishable as a result of their organizational exposure. Members answered a battery of four basic questions about their organizational acumen.[18] When respondents' knowledge is broken out by length of membership, the experiential search perspective receives strong support. Long-time contributors know a great deal more than newcomers (Table 2). Roughly one-third of the one- or two-year members missed all four of these questions; the same is true of less than 5% of the members who have been in the group for more than 10 years.

Also, as expected, the mean level of knowledge from one cohort to the next shows a clear pattern of diminishing

Table 3. Organizational Experience and Group Issue Opinions (Common Cause Members without Opinions) (%)

Issue	Organizational Experience (Years in Group) 1	2	3	4	5	6	7	8	9	10	11	12	Total Percentage	Number of Cases	X^2
Limit government spending	58	38	37	31	40	37	33	39	51	45	51	43	41	1,155	22*
Sunset legislation	40	23	20	21	11	13	9	11	6	8	14	10	14	1,164	54**
Campaign finance	24	24	11	10	6	7	5	1	2	1	7	3	7	1,167	74**
Equal rights amendment	28	30	26	19	24	19	13	18	17	12	15	13	18	1,162	25**
Lobby disclosure	20	20	14	8	5	9	5	1	3	1	11	4	7	1,174	56**

$^*p < .05$
$^{**}p < .01$

Table 4. Organizational Experience and Personal Issue Opinions (Common Cause Members without Opinions) (%)

Issue	Organizational Experience (Years in Group) 1	2	3	4	5	6	7	8	9	10	11	12	Total Percentage	Number of Cases	X^2
Limit government spending	5	8	7	7	6	9	3	2	8	5	3	6	6	1,173	9
Sunset legislation	2	3	9	2	1	6	2	1	1	3	1	3	3	1,183	21*
Campaign finance	2	1	6	6	2	4	2	1	0	1	2	4	3	1,181	15
Equal rights amendment	0	3	6	1	1	1	1	2	2	1	2	2	2	1,182	13
Lobby disclosure	0	1	2	1	0	0	0	0	0	0	0	1	1	1,186	11
Defense spending	10	10	4	11	12	9	6	3	5	5	5	10	7	1,180	15
Equality of opportunity	3	16	19	12	17	6	18	10	17	17	19	12	15	1,179	18
Social service spending	5	11	6	10	1	14	6	6	10	8	8	8	8	1,167	13
Inflation	13	22	16	22	20	18	11	16	21	21	24	19	19	1,167	11
Abortion	10	1	7	5	2	4	2	4	2	1	3	3	3	1,178	16
Minimum guaranteed income	13	8	16	14	17	14	12	18	16	16	20	17	15	1,172	7
School busing	8	15	10	14	11	6	15	15	16	17	20	13	14	1,176	12
Nuclear energy	8	10	10	14	20	11	15	11	13	11	8	17	13	1,180	13
Soviet relations	33	39	25	20	32	34	32	27	29	31	29	30	30	1,180	10

$^*p < .05$

marginal returns with organizational experience. A year has roughly four times as much impact for newcomers as for longtime members.

Next, consider knowledge about the collective goods that Common Cause furnishes. Members were asked about associational positions on five issues on the group's political agenda. In four out of five instances, new members are substantially more likely to reply that they do not know the Common Cause position on these policies (Table 3).[19] But when queried about their own positions on either these issues or other items not on the Common Cause agenda, differences between older and newer members are nonexistent, with one slight exception (Table 4).

While recent members are as opinionated about the public agenda as long-time contributors, they are less well versed in the organization's stances. A substantial portion of new members could not have been motivated to join the group by its overall issue positions, since they did not know them. This finding supports the belief that general, largely purposive benefits, rather than returns from specific collective goods, are at the heart of the initial joining decision.

The learning process is also apparently fundamental for the leap from rank and file to activist status. Stated bluntly, activists are made, not born. Modal activists have been in the organization for three years when they move up from the rank and file; a scant 10% of all members are activists from the start.[20] This behavior is consistent with a search framework. Imperfectly informed individuals join the organization, learn about it, and decide what their next step is—whether to drop out, remain in the rank and file, or step up to the activist cadre.

The learning process provides initial side evidence that experiential search is a fundamental component of organizational membership. Contributors write checks and then refine their knowledge about the organization.

Exploring the Conditional Membership Process

Quitting is an important consequence of experiential search. This perspective requires that some members leave, but the likelihood of exit should diminish with experience.

Retention decisions should be explicable using the experiential search framework. To test this, four nested models, similar to those previously discussed, are made operational: (1) the Olsonian model in which divisible benefits are assumed to exceed costs; (2) the same model with organizational experience incorporated; (3) this second specification with collective benefits added; and (4) a complete formulation that also takes into account purposive and solidary returns. Estimation of these models permits the determination of both whether organizational search is involved and what specific factors drive Common Cause participants to retain or revoke their membership.

Measurement. The likelihood of membership retention in the next contribution period—the dependent variable—is made operational using a seven-point scale. Scores range from one for those certain to quit to seven for those certain to remain. Roughly consistent with Common Cause's 78% renewal rate, 54% of all members queried responded that they were certain to renew (it should be remembered that the sample is weighted toward activists; only 49% of the rank and file expressed certainty that they would stay). The other 46% expressed different levels of uncertainty (scored from six to one): 26% called renewal very likely; 12% said it was likely; 3% suggested that they were not sure; and another 6% claimed that they were not very likely to, were unlikely to, or definitely would not renew their membership. Half of the organization was up for grabs to one degree or another, and a nontrivial minority was relatively certain of leaving Common Cause.

The factors posited to structure this choice are made operational in the following manner:

1. *Costs of membership* are measured as the ability to pay (family income) and the respondent's sensitivity to costs.[21]
2. The relevance of three *divisible benefits* are incorporated: (a) the importance of publications and whether contributions would cease without them; (b) the perceived value of political information; and (c) whether or not a member has political aspirations.[22]
3. The lure of *collective benefits* is gauged by whether individuals (a) agree with the positions of Common Cause on key issues; (b) consider the leadership effective in providing collective goods; (c) are active in the group; and (d) believe that

they are efficacious in the production of collective goods.[23]

4. *Learning* is measured by organizational experience, made operational to capture the hypothesis of diminishing marginal returns with both a logarithmic and a linear term of the number of years in the organization. Incorporating linear and logarithmic terms is a standard means of measuring diminishing marginal returns (e.g., Maddala 1977).
5. *Purposive benefits* are tapped by whether respondents feel an obligation as good citizens to participate and whether they care about the group.[24]
6. *Solidary benefits* are measured by whether the members value the interpersonal interactions Common Cause provides and a dummy variable on whether they have friends and colleagues within the organization.[25]

Given these indicators, many expectations are straightforward. Others are not as clear as they might seem and will be contingent on how well Common Cause provides benefits—for instance, the degree to which the organization satisfies members who want to promote their political careers or who seek rewarding interpersonal relationships.

One clear, important expectation is that the sign for the logarithmic version of organizational experience ought to be positive (but there is no expectation for the linear term); this would reflect the diminishing marginal impact of experiential learning. Organizational experience acts as a surrogate for the respondents' level of information and their certainty about a host of factors that are correlated with time. As the previous empirical analyses demonstrate, members gradually learn about how a group functions and slowly develop an understanding of its positions. They should also become more certain about their subjective valuation of the host of benefits that the association offers. A big advantage in employing years in the organization is that it is a continuous measure, which makes it feasible to test the hypothesis of diminishing marginal returns.[26]

It is possible to debate whether some of the other indicators measure one factor or another. Some ambiguities are inevitable, since benefit types are not empirically orthogonal to one another; and particularly in the case of collective and purposive rewards (to be discussed shortly), imperfectly informed individuals are likely to confound one benefit with another. On the whole, however, the indicators in this analysis gauge what has traditionally been meant by divisible, collective, purposive, and solidary benefits, as well as the costs of membership.

Results and interpretations. The ordinary least squares estimates for these models (Table 5) show that the retention decision is explicable.[27] Even when the focus of analysis is current members—a homogeneous, truncated sample of society—it is possible to separate out those prone to stay from those likely to leave. Such results are inconsistent with the informational assumptions underlying previous formulations of the joining process.

A framework incorporating various kinds of returns and learning does a superior job to more limited alternatives. A series of F tests comparing each model with its more restricted predecessor—model 2 with model 1 ($F_{2,1076} = 33.15$; $p < .01$); model 3 with model 2 ($F_{4,971} = 42.11$; $p < .01$); and model 4 with model 3 ($F_{4,911} = 7.93$; $p < .05$)—clearly identify model 4 as the best specification (see Pindyck and Rubinfeld 1981, 117-19, for an explication of joint F tests). Looking at selective incentives generated via divisible benefits is not enough; collective, solidary, and purposive benefits, as well as organizational learning, also affect the decision calculus.[28]

The whole gamut of costs and benefits go into the retention decision.[29] The price

Table 5. Determinants of Retention Decisions

Variable	Model 1	Model 2	Model 3	Model 4
Constant	6.25**	5.38**	2.55**	.61**
Costs				
Ability to pay	.01	-.01	.03	.03
Low sensitivity to costs	1.11**	1.04**	.83**	.78**
Moderate sensitivity to costs	.51**	.52**	.45**	.42**
Divisible benefits				
Value of political information	.01*	.01	.01	.03
Publications' value	.15**	.15**	.09**	.08**
Low sensitivity to provision of publications	.29**	.20**	.12	.08
Moderate sensitivity to provision of publications	.24**	.21**	.12	.06
Political aspirations	-.15**	-.19**	-.18**	-.11
Learning				
Organizational experience		-.03	-.02	-.02
Natural log of organizational experience		.60**	.38**	.39**
Collective benefits				
Agreement with group's positions			.01**	.01**
Assessment of leadership's achievements			.30**	.27**
Activism in group			.08**	.10**
Feeling of personal efficacy regarding group			.05**	.05**
Purposive benefits				
Care about group				.04**
Sense of citizen duty				.07**
Solidary benefits				
Value interaction				.12**
Friends or colleagues are members				.10**
Number of cases	1,114	1,085	986	930
Adjusted R^2	.18	.22	.34	.36

*$p < .10$
**$p < .05$

of membership is an important consideration, although ability to pay, per se, is not. While all members know the monetary cost of joining, some are especially sensitive to it. Those finding that membership is not worth the opportunity costs—of foregoing participation in another organization, for instance—depart and either search elsewhere or become inactive.

The salience of divisible benefits is more nebulous. When the Olsonian model is made operational the group's publications, the political information it provides, and the opportunities it furnishes to political aspirants all appear important. When other benefits are fully integrated into the decision framework, however, everything but the value of Common Cause publications is insignificant, and even the estimate of its impact is halved.

The utility of the Olsonian framework as a predictive model for retention decisions in public interest groups is questionable. As an explanatory framework, it is even less successful. This is not a direct indictment of a perspective designed to explain initial joining in economic groups. However, it does provide some reason to doubt the assumptions underlying this decision-making model.

The effect of political aspirations, although insignificant in the final model ($t = 1.3$), is nevertheless intriguing. These individuals are less likely to remain in Common Cause than other contributors. Once they learn about the true nature of the organization—its antagonistic stance toward political parties, for example—they may decide that Common Cause is the wrong place for them and move on. Even if the politically ambitious are more likely than others to join the association (and given the percentage of members who have such motivations, this is probably the case), they can still be more prone to quit. Conversely, the relationship between future aspirations and retention will be positive in organizations that are good mechanisms for building political careers.

Collective benefits appear to be an important element in the conditional joining calculus. These findings refute the assertion that members never learn about collective goods; if the latter were the case, these estimates would be insignificant. Despite the fact that few members cited such returns as the principal reason for initially joining, they seem to be the most relevant factor for the retention choice.[30] Assessment of the leadership, level of activism, and feelings of efficacy all have an impact. So too does agreement with Common Cause's positions. Of particular salience are those good government issues—sunset legislation, campaign finance, and regulation of lobbyists—in which the organization has traditionally been involved.[31]

An explanation for this tension between the apparent insignificance of collective benefits in joining decisions and their critical role for retention calculi centers on experiential search. Many people are ignorant about the organization when they first sign on but gradually learn upon joining. All they might know initially is that Common Cause is a group that deals with good government issues and for which experiential search comes cheaply. After contributing, they discover more specifically what the organization does and how much it accomplishes. These data are employed in calculating whether or not to stay in the group. Members move away from a concern about seemingly purposive benefits toward an interest in more specific collective returns.

This result implies that the traditional dichotomy between purposive and collective benefits—the former representing an adherence to the group's stated goals and the latter reflecting members' beliefs about their impacts on the production of collective goods—reflects a false distinction. The difference between these two types of rewards is overwhelmed, at least as it is tapped by survey instruments in the public interest group context, by the conditioning effect of information on the estimated policy benefits derived from membership.

Put another way, what is being interpreted as collective benefits may really be specific statements about purposive returns (this has previously been implied in Hardin 1982). Contributors' responses may reflect their perceptions of the group's, and not their personal, impact on the provision of collective benefits. Their assessment of the group's leadership and policies is important for deciding whether to exit because they think the organization can have an impact on the production of public goods. They learn about the group's efficacy and how their policy preferences correspond to the organization's and either stay or depart accordingly. Learning integrates initial and conditional membership: broad motivations are replaced by more specific ones. Group leaders have an incentive to foster the confusion between individual and associational efficacy. To the extent that they control the information contributors employ to update their cost-benefit calculi, the elites will add to the confusion by telling members that they make a difference by acting collectively.

Explicitly purposive benefits are still germane. Common Cause is a good place for people who really care about such returns and want to be good citizens. Even broad policy attachments to the group may make participation satisfying. Consequently, contributors who have an abiding interest in being good citizens or who develop a strong identification with Common Cause tend to keep on giving.

To summarize, these findings about collective and purposive rewards support the proposition that members go from general to specific reasons for staying or leaving as they become more knowledgeable. This explanation is consistent with the side evidence about learning. All that is additionally required is the assumption that individuals recognize their informational shortfalls before joining and employ experiential search to remedy them partially. This strategy leads members to offer vaguely purposive reasons for initially joining and more specific concerns about collective goods for staying or leaving.

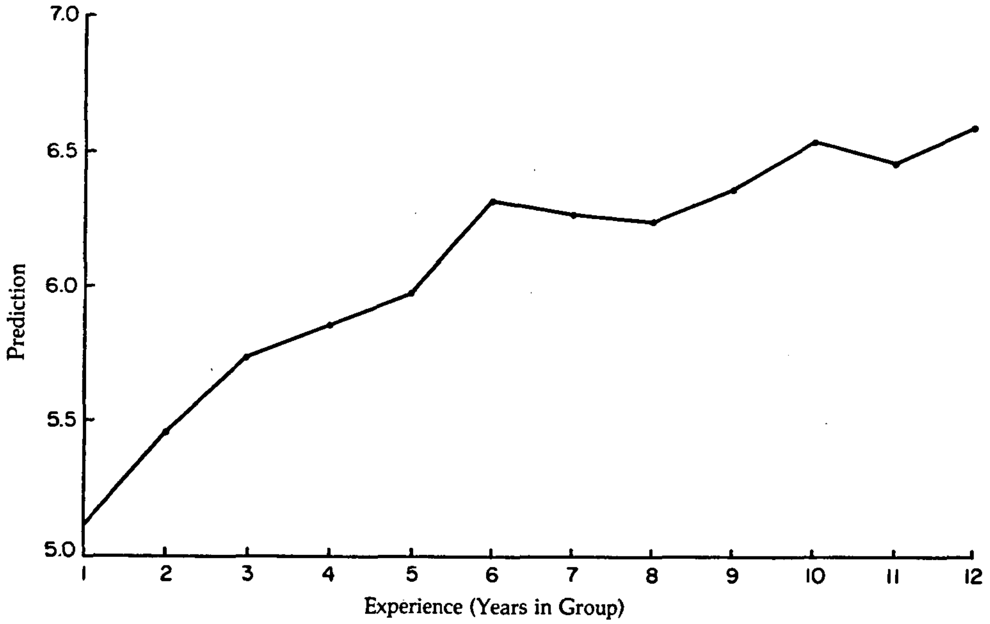
Similarly, solidary benefits are rarely mentioned as a major reason for joining. Yet they too are significant factors in the retention choice. For those seeking such interactions, the organization delivers the goods. Others may discover that a by-product of searching over purposive and collective benefits is a rewarding associational involvement. Again, the tension between initial and subsequent conditional membership choices stems from the fact that contributors learn over time. Those who find rewarding interpersonal relationships stay in Common Cause; those who either do not care about such interactions or decide that the organization does not provide the solidary benefits they desire, depart.

The findings regarding learning offer a second test of the experiential search theory and, once again, provide validation. Specifically, they lend credence to the hypothesis that experiential learning yields diminishing marginal returns. Each year has a positive, yet declining, impact on the probability that members will remain committed even after all the standard costs and benefits of joining are incorporated into the model. Ceteris paribus, a newcomer scores three-quarters-of-a-point lower on the seven-point scale than the most veteran contributors. In other words, there is considerable vacillation among new members about their future intentions, but this uncertainty dissipates over time. By and large, departing contributors are recent converts who, upon learning about the group, become disenchanted; they are not long-term members who grow bored with Common Cause.

Similarly, the *overall* predictions from the full experiential search model (model 4) lend credence to the hypothesis that the probability of staying increases temporally but at a diminishing rate (Figure 2). The impact of the early years is roughly five times greater than the effect of the later years; and, as discussed previously, there are no obvious differences between long-term members and newcomers that might render this relationship artifactual. The only possible inference is that individuals learn and update their information. Those who like what they see stay, and the rest search elsewhere. This conclusion is consistent with Common Cause's own troubles in holding onto new members. Only about 55% of first-year members continue to contribute the following year, while roughly 90% of long-time members remain.

Finally, consider what happens when the sample is split between newcomers—those contributing for six years or less—and veterans and a revised version of model 4 is estimated (Table 6).[32] Most strikingly—but predictably from an ex-

Figure 2. Retention Predictions and Organizational Experience



Note: Predictions are based on the seven-point scale used to measure a respondent's probability of retention. Scores range from one (certain to quit) to seven (certain to renew).

periential search perspective—the model does a superior job of explaining why newcomers come and go as compared to veterans. Longer-term members more closely approximate the full information ideal and are prone to depart for idiosyncratic reasons. Also as predicted, specific characteristics loom larger in newcomers' retention decisions.

While there is no appreciable difference between the two samples in the findings for purposive benefits (no coefficient is significant), there are variations in the effects of solidary benefits and especially collective returns. In the latter the impact of these factors is stronger for newcomers than for veterans. Not only is learning about specific characteristics crucial, it is especially salient for those who are new to the organization.[33]

All three tests of the experiential search perspective support its validity as a superior framework for conceptualizing the retention choice. Members make their decisions in a systematic, comprehensible fashion. The decision to remain in the organization reflects their discovery that the group provides the benefits they are searching for. Learning is an important component in understanding how conditional membership choices are made. Overall predictions about the probability of remaining in the organization reflect the diminishing marginal returns to be expected if individuals garner information through experiential search. An individ-

Table 6. Retention Decisions of Shorter- and Longer-Term Members

Variable	Newcomers	Veterans
Constant	−.23	2.06**
Costs		
Ability to pay	.04	.01
Low sensitivity to costs	1.12**	.56**
Moderate sensitivity to costs	.58**	.23**
Divisible benefits		
Value of political information	−.01	.06
Publications' value	.12*	.07*
Low sensitivity to provision of publications	.29*	.10
Moderate sensitivity to provision of publications	.09	−.03
Political aspirations	−.19	−.10
Learning		
Organizational experience	.10**	.04*
Collective benefits		
Agreement with group's positions	.01**	.01**
Assessment of leadership's achievements	.28**	.24**
Activism in group	.16**	.08*
Feeling of personal efficacy regarding group	.02	.06*
Purposive benefits		
Care about group	.05	.04
Sense of citizen duty	.10	.04
Solidary benefits		
Value interaction	−.04	.13
Friends or colleagues are members	.20**	.06
Number of cases	370	560
Adjusted R^2	.40	.24

*$p < .10$
**$p < .05$

ual's first years in the organization are especially important—particularly for learning about highly specific characteristics.

Conclusions: Experiential Search and the Retention Choice

Understanding why people leave or remain in groups has received little attention. The retention decision should not be taken as a given, that is, something that is determined by the initial choice to contribute. The conditional membership decision is fundamental for organizational stability and is a constant source of anxiety for group leaders. It reflects an experiential search process through which contributors acquire information and make increasingly knowledgeable choices.

This perspective is very different from either a game-theoretic framework or those decision-theoretic models adopted by previous analysts of organizational membership. Here it is assumed that imperfectly informed decision makers are aware of their shortfalls and take them into account in making choices while playing against nature. Because accruing information is costly, there is an incentive to become informed in the most efficient way possible. If the price of membership is small and a mistake is easily rectified at the next contribution period, joining a group and learning about it experientially may well be the optimal strategy.

Common Cause members behave very much in this manner. The following picture of the average contributor emerges from this analysis: Individuals join an organization about which they are largely uninformed. They have a rough idea about what it stands for, but they lack the detailed knowledge needed to decide whether this is the best association for them. They are as informed about politics, as educated, and as wealthy as long-time members. But they lack organization-specific information, which is best gained experientially.

Consequently, the retention process is explicable by specifying a model that reflects information updating. Among other things, the findings make clear that early on when the incremental informational gains are greatest, there is a higher probability that revising prior beliefs will precipitate departures. As time progresses and the additional impact of another period diminishes, so too does the probability of dropping out. The accrual of knowledge about the organization also leads members to rely upon more specific criteria in making their conditional membership decisions than they claim motivated their initial contributions. Examining the model's overall predictions about future membership behavior simply buttresses the experiential search story further. So too does the contrast between what the model reveals about newcomers and about veterans.

Group participation ought to be conceptualized as an experiential search process in which individuals with only imperfect information are forced to make choices. They may commit calculated mistakes, but they will eventually rectify them. This framework makes organizational quitting understandable and provides a linkage between the initial membership choice and follow-up decisions. It furnishes insights into how the ebb and flow of group membership ought to vary systematically across associations, depending upon both the costs of contributing and the variety of benefits available to association members. Any number of interesting comparisons for testing this general perspective spring to mind: for example, contrasting an organization like Common Cause which conducts much of its business through the mail and over the telephone, and one like the League of Women Voters, for which interpersonal interactions that provide solidary rewards are more central.

The empirical findings suggest that group leaders might weigh newer members' preferences more heavily than those of long-time contributors, assuming that both are equally valued. They also imply that any educational efforts should be centered on appealing to these newcomers; utilizing scarce resources to sway veteran participants is likely to be a less productive investment.

What is needed in the future is the collection of data better designed to capture the dynamics of retention choices. Without a doubt, the key flaw of the present research is that only limited temporal data are available. Future endeavors must employ longitudinal designs, including studies that follow up on individuals who begin, continue, and cease contributing (and perhaps go elsewhere); analyses that explicitly build in samples of the general public as well as group members; and research that incorporates a multiplicity of groups varying on those dimensions that should affect the amount of experiential search undertaken. Such data will provide a better, more integrated understanding of why people join and either stay in or leave organizations than the initial attempt made in this analysis.

Notes

Earlier versions of this research can be found in California Institute of Technology Social Science Working Paper no. 651 and in a paper presented at the 1987 annual meeting of the Southern Political Science Association, Charlotte, NC. The assistance of John Aldrich, Kim Border, Bruce Cain, Jeffrey Dubin, Thomas Gilligan, Jonathan Nagler, Barbara Rothenberg, and John Wright is appreciated.

1. Building a core membership is extremely important for ensuring long-term organizational survival. Even those associations that receive foundation grants and support from other organizations may find that these sources of income are unreliable.

2. In her study of four Michigan groups, Cook (1984) does provide some data on whether member satisfaction-dissatisfaction is associated with intentions to remain in the organization. Moe (1980) also touches on the decision to exit in his empirical work. In an intriguing theoretical piece, Johnson (1987) demonstrates that if a median voter rule were employed in determining dues levels (contributors constitute the electorate), voluntary associations would inevitably collapse due to exit. He also correctly notes, however, that group leaders can thwart this potential instability. At Common Cause, for example, dues are set by those running the organization. In his well-known work, Hirschman (1970) acknowledges that individuals may respond to changes in organizational performance by departing. However, his model is based on the idea that deteriorating leadership performance over time may precipitate either exit or voice from individuals possessing perfect information. By contrast, the present research explores the implications of imperfect information for members' behavior.

3. *Retention* is the term utilized in studies of reenlistment decisions by military personnel (e.g., Gotz and McCall 1983, 1984).

4. This is a generic problem that all organizational leaders face. Employers, for instance, also deal with the problem of holding onto valuable workers, as well as attracting capable replacements. What sharply distinguishes interest groups from other organizations is that nonparticipation in the public arena is a far more viable alternative.

5. The Common Cause survey was conducted in the fall of 1981 by the political science department of Stanford University. It was funded by grant SES-8105708 from the National Science Foundation to Heinz Eulau in support of research by Jonathan Siegel. Many thanks to Siegel for generously sharing these data. Because this is a stratified sample, the descriptive information is frequently broken down between rank-and-file members and activists; but, to reiterate, the sample design has no impact on the hypothesis tests conducted. Only if the sample is stratified on the choice in question—for example, the decision to join—is there a problem.

6. Not all of the alternative specifications are examined here. For example, Smith (1985) develops a model of contributions to environmental organizations based on the theory of club goods. However, none of the public goods that Common Cause (or few other groups) might provide have an element of excludability to them, which is essential for translating the economic theory of clubs into one of group membership. Similarly, Hansen (1985) furnishes a context-dependent model of membership decisions based on prospect theory. Incorporating his insights would require a complete panel design.

7. Although the traditional distinction between purposive and collective benefits is employed at this point in the analysis, a different perspective on this dichotomy will be introduced later.

8. Moe takes the imperfect information assumption somewhat further, but almost strictly from the perspective of group leaders (particularly, how to structure an organization to encourage donations) rather than from the contributors' viewpoint.

9. An interesting implication is that fewer members will learn experientially in groups with expensive membership charges. Instead, potential contributors will use their resources to search from outside of the organization. Membership in interest groups with relatively high dues should therefore be less volatile than in comparatively "cheap" organizations.

10. A quintessential example of exogenous forces is found in Wilson's (1962) observation that "amateur Democrats" lose their enthusiasm to spend endless hours working for the cause once an election is over.

11. As will be discussed in more detail, the ideal means of studying membership would be through a panel study. The analysis presented here is a second best alternative.

12. Respondents were asked whether their present family income was (1) under $10,000; (2) $10,000–$20,000; (3) $20,000–$25,000; (4) $25,000–$35,000; (5) $35,000–$50,000; (6) $50,000–$75,000; (7) $75,000–$100,000; or (8) over $100,000.

13. Seventy-three percent of those contacted actually wrote: 60% of the rank and file and 90% of all activists. Further corroboration of the mobilization of the membership is given by the finding that 44% of those sampled (35% of the rank-and-file members contacted and 78% of the activists) reported writing in the previous year about at least one of the following six issues: a constitutional amendment limiting government spending, sunset laws, campaign finance, reapportionment, lobby disclosure, or the Equal Rights Amendment (ERA).

14. An overwhelming 98.5% of those queried reported reading the Common Cause publications they receive.

15. The 31 reasons for joining listed in Table 1 were coded from 99 different answers. Only those citing a specific issue or issues, leadership, nonpar-

tisan group, or unique-important were subsequently difficult to classify into the four broad categories. They accounted for about 8% of the respondents.

16. Empirically there should be (and is) a positive relationship between knowledge—and organizational experience generally—and the decision to stay in the group. Those who gain information and do not like what they learn drop out.

17. A strong caveat is in order: Cross-sectional evidence is being used to draw temporal conclusions. It is nevertheless hard to tell a compelling story about how employing this cross-sectional information would confound this part of the analysis. Thus it is worthwhile to try to uncover evidence of contributor learning with these data, especially because they are probably the best currently available.

18. The four statements (with their answers), to which members could reply *true*, *false*, or *don't know* are (1) members of Common Cause elect the governing board [true]; (2) members of Common Cause elect the Common Cause chairman [false]; (3) Common Cause is a federation of state and local organizations [false]; and (4) Common Cause state organizations determine their own issue agendas [true]. A further check was performed to make sure that the relationship between knowledge and tenure is not a spurious reflection of the fact that activists have been in the group longer. The data show that even after controlling for whether respondents are activists or rank-and-file members, the association remains quite strong, although activism also is related to organizational knowledge.

19. The one exception dealt with constitutional amendments limiting government spending. This issue was relatively new on the Common Cause agenda, and long-time members themselves might not have had time to learn about it. More than 40% of the entire membership had no opinion on Common Cause's position on it, as compared to a maximum of 18% on the other four items.

20. Rather than being asked how many years they have been activists, these members were requested to choose one of five categories: (1) 8–10 years, (2) 6–7 years, (3) 4–5 years, (4) 2–3 years, and (5) 1 year or less. The three-year estimate is conservative; it is based on the greatest possible number of years—10, 7, 5, 3, or 1—that participants might have been involved relative to the number of years they have been in the group. If the means of the categories were substituted for the upper bounds, modal activists would be in their fourth year.

21. As mentioned, family income is tapped with an eightfold variable. Cost sensitivity is gauged with two dummy variables coded from a question in which members were asked if they would remain in the organization *(yes, can't say, no)* if Common Cause raised its annual dues from $20 to $40.

22. Members were asked two fivefold questions about the importance to them personally of (a) magazines and other Common Cause publications and (b) the political information the association provided. They were also given a question parallel to the one on cost sensitivity in which they were queried whether they would stop contributing if the publications were halted.

23. Position agreement is measured as

$$-1\,[\sum_{i=1}^{5}(X_{ip}-X_{ic})^2],$$

where X_{ip} and X_{ic} are, respectively, contributors' personal preferences and views of where Common Cause stands on the following issues: an amendment limiting government spending, sunset legislation, campaign finance laws, the ERA, and lobby disclosure laws. Of course, only those respondents with personal preferences and estimates of Common Cause positions are included. Contributors without issue opinions (see Tables 3 and 4) are excluded. Leadership assessment is an additive index combining fivefold responses to questions about the legislative success of Common Cause and an explicit rating of how well the leadership and staff do their jobs. Activity is tapped by counting whether a person has (a) written or talked to Common Cause staff or leaders about a group policy or position; (b) attempted to attract new contributors; (c) attended a local Common Cause meeting in the last year; (d) voted in the 1981 governing board elections; or (e) completed the 1981 membership poll. Finally, the personal efficacy measure combines the responses to two parallel questions on how important to the success of Common Cause individuals think their own contributions and their own political activities are.

24. The former is an additive index of the importance assigned to membership in Common Cause as a means of fulfilling the responsibilities of citizenship, supporting leaders like John Gardner and Archibald Cox and helping to ensure good government. Caring is an additive scale of member interest in each of the five issues used to construct the position agreement scale.

25. The former is a fivefold response to a question on the importance contributors attribute to meeting interesting people and making new friends.

26. It might be maintained, in the spirit of the so-called garbage can theory (Cohen, March, and Olsen 1972; March 1978), that organizational experience is tapping the adaptation of preferences to what Common Cause has to offer, that is, the conventional assumption that preferences are fixed may be incorrect. Admittedly, it is impossible to distinguish definitively between changing preferences and learning given the lack of panel data. However, the fixed preference assumption is probably quite reasonable. It has been shown in this analysis that individuals do learn about the organization's operations and issue positions. It is also hard to believe

that Common Cause members suddenly develop a preference for good government, political information, or social interactions once they sign up for the group.

27. Before estimating the models, a number of preliminary steps were taken. To ensure that it is acceptable to pool the Common Cause-designated activists with rank-and-file members a Chow test (Chow 1960) was conducted. The results of the test proved to be insignificant, thus permitting the pooling. Also, Hausman tests (Hausman 1978) were utilized to ascertain whether any of the independent variables were endogenously determined (the logarithmic and linear organizational experience terms were tested jointly): none were. Additional tests were undertaken to investigate whether the standard sevenfold specification of the dependent variable was correct. A Hausman test for determining whether the slopes for a seven-ordered probit are identical to those for a threefold analog (*no, can't say, yes*) led to the rejection of the null hypothesis that $\mathbf{B}_7 = \mathbf{B}_3$ ($X^2 = 35$, $df = 19$). This finding implies that the seven-category operationalization is subject to specification error of some sort, although the ramifications are uncertain. This discovery precipitated a further investigation to uncover whether a better specification was available. The obvious alternative is a two-stage conditional structure that breaks the retention decision first into a direction (*no, can't say, yes*) and then into a strength (*certain, very likely, likely*) choice. When the log-likelihood ratios of the conditional and unconditional processes are compared (see Vuong 1986), however, the latter model is far superior. Given this strong finding, the unconditional, sevenfold specification was adopted with the caveat that there might be some superior alternative. Having decided upon this specification, both ordinary least squares (OLS) and probit estimation were employed. The results are identical for all intents and purposes; the OLS results are reported here due to their ease of interpretation. To reiterate what has been noted previously: attrition should not have had an impact on the estimates in Table 5. As long as quitting is related to exogenous factors—unhappiness with the group, evaluations of the leadership, and so on—and these factors are controlled for, the ensuing estimates are unbiased. None of the difficulties associated with choice-based sampling are relevant in this case.

28. Multicollinearity is not a major problem in these models. The linear and logarithmic learning terms are highly correlated. This intercorrelation is to be expected but, as mentioned, utilizing a logarithmic and a linear term was suggested to tap diminishing marginal returns (see, for example, Maddala 1977). If one of the two terms is excluded, the other findings do not change substantially. On examining the intercorrelations between the independent variables, only two other pairs of correlations are above the .30 level: the valuation of political information and publications (.59) and the two dummy variables on cost sensitivity (−.60). Combining the information and publication variables results in a poorer-fitting model. The resulting variable is insignificant (whereas the publications' value is significant in Table 5), and no other substantive results change. There is also no appreciable change if the variable measuring moderate sensitivity to costs is dropped except that the coefficient for high sensitivity increases in magnitude. To explore more systematically the potential for multicollinearity, the singular-value decomposition (SVD) recommended by Belsley, Kuh, and Welsch (1980) was performed for model 4. Interestingly, the SVD for the full model shows a moderate level of multicollinearity (SVD = 60). However, a major cause of this multicollinearity is the relationship between the intercept term and other variables: this result is common when using dummy variables but does not harm the estimates. If the intercept and the logarithmic learning terms are dropped, the SVD is cut in half, to a level where experimental work shows the multicollinearity is quite small (SVD = 30). Again, the findings are robust with respect to multicollinearity.

29. Unfortunately, no good measure of nonpecuniary costs was available for the whole sample. An indicator that worked for part of it ($N = 395$) was whether or not Common Cause members responded to attempts to mobilize them. Even after controlling for all other factors, these costs are important determinants of the retention decision. This result provides evidence that those who find these additional organizational demands taxing depart, presumably either to find an association where they can simply write checks or to leave the world of organizational participation completely.

30. An increase of one standard deviation in a member's score on each of the collective benefit indicators would result in a jump of .71 on the retention scale. This change is slightly greater than the impact of membership costs (.63), followed by divisible benefits (.23), purposive returns (.21), organizational experience (.18), and solidary rewards (.17). The strength of membership costs might be a bit of a surprise. However, the measures of cost sensitivity—the willingness to quit in response to a $20 increase in dues—are also tapping estimates of the value of benefits.

31. These findings are based on a regression using the model 4 specification, with the sums of the squared differences for each issue being substituted separately instead of cumulatively. The two relatively new issues on the Common Cause agenda—ERA and placing limits on government spending—are not relevant. These concerns represent a departure from the issues for which the organization gained its reputation and apparently have not become a prime reason for remaining in the organization (or for joining, probably).

32. The logarithmic term for learning is now unnecessary, since the sample was split according to how many years a contributor was in the group. The definitive means of testing whether coefficients vary between newcomers and veterans is to employ interaction terms with the full sample. However, multicollinearity makes such estimates unfeasible in this instance.

33. The larger impact of cost sensitivity on newcomers than veterans probably reflects the former's greater uncertainty about the value of membership.

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MASS DYNAMICS OF U.S. PRESIDENTIAL COMPETITIONS, 1928–1936

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This analysis investigates the mass dynamics of competitive electoral politics with respect to the presidential vote during the 1928–36 realigning period for the United States. A formal system of three interdependent differential equations is employed to characterize the dynamic processes of the aggregate voter shifts between the Republican, Democratic, and nonvoter populations. The modeling strategy is used to locate substantial variations in the mass dynamics between large subgroups in the electorate as well as to identify both national and socially interactive components to the patterns of voter movements. The results show that the overall realignment period was quite complex. Vote switching from the Republican party to the Democratic party was the dominant characteristic of the 1932 election, whereas additional Democratic gains in 1936 came mostly from new voters.

This study examines the roles played by new voters and former Republicans in the dramatic rise in Democratic presidential support between 1928 and 1936. The analysis focuses on the process of deinstitutionalization among voters in a setting of large-scale new voter participation. The term *institutionalization* refers to a constraining influence on electoral competitions and is linked substantively to work by Przeworski (1975) and, to some extent, Huntington (1968). The institutionalization of partisan behavior is identified by the existence of enduring partisan attachments and is viewed as a function of time and the frequency of repeated electoral experiences among voters (i.e., the development of a party "habit"). *Deinstitutionalization* refers to the process by which voters break these former partisan bonds.

There are two factors crucial to the process of deinstitutionalization as it is characterized here. First, the assumption is made that influences that disturb partisan ties affect voters differentially, depending upon the force of the political appeals on particular groups in the population and the strength of each group's previous ties (a central theme in work by Nie, Verba, and Petrocik 1979). Second, previous electoral experiences for certain groups can produce resistance to these appeals. This second factor addresses the concept of political immunization as it has been described by McPhee and Ferguson (1962). Major social and political disturbances of sufficient magnitude to cause large-scale changes in partisan attachments among many groups should affect more strongly those who are the least immunized to alternative partisan appeals.

Critical to the dynamics of the deinstitutionalizing process is the role played by new voters, a totally noninstitutionalized and nonimmunized sector of the electorate. A number of questions on the general impact of new voters on electoral

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systems bear particular relevance to this analysis. When new voters enter the electorate in substantial numbers in response to a major social disturbance (such as the Depression), do they do so equally across all subgroups in the population? In terms of the timing of their entry into the electorate, do they begin to participate at moments when there are large partisan shifts as well? Alternatively, are they drawn into the electorate following a previous election in which there was significant excitement generated by large partisan shifts? Or, perhaps, do the new voters enter the electorate in massive waves due to exogenous social and political conditions, subsequently destabilizing the existing partisan alignment and precipitating a full-scale realignment? Answers to these questions are viewed here as central to an understanding of realigning processes and are addressed with regard to the 1928–36 realignment period in the United States.

In characterizing the dynamic processes involved during periods of volatile electoral change, this analysis distinguishes between two separate mechanisms by which such mass movements may occur. The first concerns the ability of nationally distributed political appeals to effectively channel the shifting partisan and new voter movements. The second addresses the ability of localized political forces to act as an intermediating factor in determining the magnitude and direction of the electoral changes. For example, in the case of the new voters, this asks the question of whether new voters are activated by national political forces as presented to them, say, by the national media or whether they are drawn into the participatory setting by other voters and localized partisan campaign forces. Here questions associated with the identification of the relative impacts of these two different political mechanisms of the mass dynamics focus on their independent effects on the new voter and shifting partisan populations.

This analysis begins with a consideration of the conversion-versus-new-voter hypotheses with regard to the 1928–36 electoral period in the United States. It presents a baseline description of the partisan strengths for the United States during the period from 1920 through 1936. It presents and investigates a formal model capturing the dynamics of the partisan competitions and new voter fluctuations and uses the model to identify contrasting patterns in roles played during the 1928–36 realigning period by certain groups in the U.S. population. Then, the magnitudes of the national as well as the more localized political influences are compared to determine the relative strengths of the underlying components of the mobilization and conversion processes.

The Conversion and New Voter Hypotheses

The literature focusing on the conversion-versus-new-voter hypotheses is quite divided. Sundquist (1983, 229–39) uses selective aggregate data to argue that Republican-to-Democratic conversion was the dominant type of electoral activity for the period. Further support for the conversion hypothesis comes from Erikson and Tedin (1981)—who use the *Literary Digest* "straw poll" data—as well as from Key (1964, 523–35), Burnham (1970), Ladd and Hadley (1978), and others. The most prominent proponent of the new voter hypothesis has been Kristi Andersen (1979b), with support for the hypthesis also coming from Converse (1975), Campbell and his colleagues (1960, 153–56), Petrocik (1981, 55–57), and others.

Arguments supporting the conversion hypothesis typically suggest that the steadily worsening national economic conditions of the time produced widespread discontent with the Republican

Table 1. U.S. Presidential Vote As a Proportion of Total Eligibles, 1920–36

Vote	1920	1924	1928	1932	1936
Democratic	.15	.13	.21	.30	.35
Republican	.26	.24	.30	.21	.21
La Follette		.08			
Total vote	.44	.45	.52	.53	.57
New voters		.004	.079	.006	.045

administration. Moreover, Roosevelt was able to mobilize many of these discontent former Republicans. Indeed, he developed a "natural" constituency among the working class, especially coming from the economically hard-hit industrial urban areas (Sundquist 1983, 214–23). The new voter proponents argue that partisan attachments were probably as firm (or nearly as firm) then as they have been found to be in later years (e.g., as reported by Campbell et al. 1960). Thus the new Democratic support probably came from a generation of new voters rather than disenchanted Republicans. Many of these new voters were young, and perhaps also immigrants or descendants of immigrants (see Andersen 1979a, 39–52; Petrocik 1981, 55).

Arguments concerning virtually all of the above hypotheses have relied on data around whose quality and completeness there are controversies. Andersen's use of the 1952–72 Survey Research Center/Center for Political Studies data to reconstruct the partisan voting habits of the respondents in terms of how they voted in the 1920s and 1930s has been seriously challenged by Erikson and Tedin (1981), Sundquist (1983, 229–39), Niemi, Katz, and Newman (1980, 648), and Reiter (1980). Erikson and Tedin (1981) have tried to address this problem by using the only available survey data for the period. However, they acknowledge that the "straw poll" data collected by the *Literary Digest* is of a "tarnished sort" (Erikson and Tedin 1981, 952). Problems of sampling technique abound, and the authors disagree with Shively's characterization of the nature and direction of the various biases (see Erikson and Tedin 1981, 953 and Shively 1971–72, 62). To date, users of aggregate data have hardly fared better. Previous studies have typically limited their analyses to particular geographical areas. The widespread use of "heuristic" samplings of aggregate data to study realignments goes back to Key (1955). For example, Key uses data for various towns in five states, and Sundquist (1983, 236–37) uses selected county and town data from five states. Yet there is no guarantee that the selected areas are representative of the nation as a whole. In sum, there are no reliable survey data for the period under study, and until now there has never been available for analysis a single *complete* set of electoral and census data for all of the United States using (the relatively small) county-level aggregations.[1]

Table 1 presents the aggregate electoral strengths for the Democratic and the Republican parties in the presidential contests from 1920 through 1936. All of the results in Table 1 are written as proportions of the eligible electorate and thus can be understood as measures of mobilization. These measures differ from those commonly presented elsewhere. Typically, the electoral outcomes of the period are represented either as vote shares (i.e., as proportions of the total vote) or as partisan vote totals. For example, vote shares are used by Ladd and Hadley (1978, 43) and Key (1964, 523–40), while partisan raw totals (i.e., actual votes) are presented by Andersen (1979a, 29).

There are problems with the vote share and raw totals methods, however. Vote shares can produce a misleading interpretation of overtime aggregate change in situations in which the total vote is also changing. For example, it is possible for a party's share of the vote to decrease due to an expansion in the denominator (i.e., the total vote) while at the same time the party has not lost any of its previous support from the population. In a situation in which new voters are suspected of playing a pivotal role in determining the direction of a shifting partisan balance, a vote share measure can indicate partisan shift when there may have been only new voter movement. The use of actual votes, or raw totals, instead of vote shares encounters a related problem. Comparing raw partisan totals at different points in time makes most sense when the size of the electorate is stable. Such a longitudinal comparison can be misleading in situations in which the electorate (i.e., total eligibles) is expanding or contracting. Thus a party may receive 15 million votes in one election and 16 million votes eight years later, but if the eligible population has increased as well, the increased partisan total need not indicate increased partisan "power" relative to the other parties or the overall electorate. Indeed, the party's support relative to the size of the total electorate could have decreased. Both of the problems mentioned above regarding the vote share and the raw vote measures are unraveled by using the mobilization measure.

The data in Table 1 indicate that both the Democratic and the Republican parties increased the level of support they received from the pool of total eligibles from 1920 through 1928. However, from 1928 to 1932, the positions of the two parties relative to each other virtually reversed. While the Democratic party increased its share of the total electorate from 21% to 30%, the Republican party's support fell from 30% to 21%. Note also that the total vote relative to the electorate increased only slightly from 1928 to 1932. Between 1932 and 1936, however, the total vote did increase substantially (more than four times the 1928–32 increase); while at the same time Democratic strength continued to increase, and Republican strength remained approximately constant. While conclusions regarding partisan shifts can barely qualify as tentative with such large aggregate measures, on the surface at least these results seem to suggest that new voters may not have played as crucial a role in the 1932 election as compared with the election of 1936 and that there indeed may have been an actual realignment in 1932, defined in terms of partisan conversions. The analysis that follows pursues these points more directly.[2]

The Structure of the Party Competition

The model of partisan competition developed below is a time-dependent system of three interconnected differential equations, which, in combination, address expectations of change in aggregated partisan electoral totals. Such models have been successfully exploited in the social-scientific literature by Coleman (1964, 1981), Simon (1957), Przeworski and Soares (1971), Przeworski and Sprague (1986), Sprague (1981), Tuma and Hanna (1984), Luenberger (1979), and Huckfeldt, Kohfeld, and Likens (1982), as well as others and have been notably useful in modeling the dynamics of military spending in competitions between nations (see Gillespie et al. 1977; Ward 1984). This type of modeling is also similar to that employed to characterize time-dependent ecological systems of biological populations within fixed environments (see May 1974). The mathematical theory underlying the analytic and dynamic properties of all such systems, both linear and nonlinear, is complete (Hirsch and Smale 1974; Kocak 1986; Luenberger 1979). Here we are interested in modeling the population fluctuations of three groups in the political environment: Democrats, Republicans, and nonvoters. We want a mathematical statement that corresponds to each group and describes change as a function of existing voter support for all other groups. Thus we desire three statements that when taken together describe the voter movements among parties as well as shifts between parties and nonvoters, all of which take place simultaneously at each point in time.

We begin by developing a model of the mass electoral dynamics for the Democratic party. In this analysis, we differentiate between two types of dynamic processes that could lead to growth or decay in the various partisan populations. The first type to be developed below is labeled the "uniform" component to aggregate partisan movements. In 1932 it is likely that many voters throughout the nation were energized through a national appeal to the electorate's broad social sensibilities. For example, widespread discontent with the Republican handling of the economy could have led to the development of a generalized sympathetic ear across the electorate for the Democratic message. Moreover, this increased attentiveness for the Democratic appeal could have affected many voters independently of their localized partisan environment (i.e., "uniformly" across the nation); that is, it may not have mattered whether or not there existed a strong Democratic presence within the voters' localized milieu. Rather, a certain number of Republicans, based only on the number of Republicans available within a given area, would have weighed their electoral options and decided to switch their support from the Republican party to the Democratic party.

If we use the proportion of the localized electorate that supports the Republican party as a measure of the size of the Republican population (theoretically available for conversion to the Democratic party), then we can express the change in support for the Democratic party as

$$dD/dt = fR. \qquad (1)$$

Here, R is the proportion of the electorate supporting the Republican party, D is the proportion of the electorate supporting the Democratic party, and f is a parameter of the model that corresponds to the probability of the occurrence of a Republican-to-Democratic conversion within the population at an instant in time. Throughout this analysis, this type of dynamic process is referred to as the "uniform" component of the model, due to the sense of the voter calculations assumed and the independence of the above mathematical statement from the strength of the localized Democratic presence, which might have conditioned, through social interaction with the existing Democratic environment, the rate of conversion to the Democratic party.

Yet this suggests another way in which partisan conversions may take place. The rate at which the national Democratic appeal produces new Democratic converts could have a social (i.e., contextual) component as well; that is, voters may see how others in their local environment react to the Democratic message. In areas in which there exists at least a moderate Democratic presence, pro-Republican sentiments on the part of some voters might be more difficult to defend. The availability of accepted alternative partisan perspectives within politically heterogeneous neighborhoods, combined with the institutional strength of a locally stronger Democratic party's electoral apparatus, should be enough to loosen the grip of the Republican party on some voters and thus allow for a higher level of conversion to the Democratic ranks. Thus we wish to capture in our modeling efforts the effects of interacting partisan populations in producing electoral change.

The above argument can be included in the model for change in the support for the Democratic party over time by rewriting equation 1 as

$$dD/dt = fR + bRD,$$

where b is a parameter in the model and expresses the probability of a Republican-to-Democratic conversion at an instant of time due to the conditional strengths of both partisan populations interacting simultaneously. This is the second type of dynamic process to be included in the model, and it is referenced below as the "social" component. It addresses the dynamics of locally interacting partisan populations, and its inclusion here reflects the lessons of some recent research indicating that the impact of such localized social forces on voter responses to electoral battles can be substantial in magnitude and crucial to the specification of the properties of aggregated partisan movements (Beck 1974; Brown 1987; Huckfeldt and Sprague 1987, 1988; MacKuen and Brown 1987).

All such interaction terms, as specified above, are symmetric in the sense that interactive growth for the Democrats is low when either there are (1) few Democrats interacting with few or many Republicans or (2) few Republicans interacting with few or many Democrats. Interactive growth is greatest when there are sizable Democratic and Republican populations, which is both an expected and desirable property of the above characterization of the partisan competitions. The above specification of such an interaction is well represented in an extensive social-scientific literature on communication, contagion, and diffusion modeling (Coleman 1964; Huckfeldt 1983; Huckfeldt, Kohfeld, and Likens 1982; McPhee 1963; Przeworski and Soares 1971; Rapoport 1963, 1983; Simon 1957; Sprague 1976). Such terms are also common in ecological models of interspecies interactions within contained biological ecosystems (Danby 1985; Haberman 1977; May 1974; Rosen 1970).

Democratic party electoral strengths can also improve due to an infusion of new voters to the party ranks. This addresses the hypothesis that the mobilization of new voters was a crucial factor in the changing Democratic fortunes between 1928 and 1936. It is possible for the new votes to be mobilized due to a national appeal to their potential partisan sensibilities, thus attracting new voters in correspondence to the size of the localized nonvoting population. Struck by the declining national economic fortunes and the possibility of change given by a new Democratic leadership, perhaps many former nonvoters made the decision to begin to participate in the partisan contests.

However, new voter movements need not have been limited to a response to a national appeal, the "uniform" component of the model. Previous nonvoters could also be motivated to vote due to interactions with partisans. Such interactions can be both direct and indirect. The effectiveness of an established partisan apparatus in mobilizing the vote by establishing direct contact with potential voters is a result that was reported long ago by Gosnell (1927) and evidenced repeatedly since. Partisan interactions with the nonvoting population leading to the mobilization of new voters can also be indirect, however. By themselves, nonvoters can witness the partisan characteristics of their communities. In some areas in which Republican norms are very strong or, perhaps, where overall voting activity in either partisan direction is very slight (i.e., turnout is traditionally low), it may be that nonvoters lack the relevant localized partisan cues that would cause them to initiate the internal processes of becoming politically involved. However, in areas in which Democratic partisan activity is already established, some nonvoters may follow the lead of their local political environment (keeping up with the Joneses, so to speak) and begin their trek to the polling booths. This addresses the "social" component of the new voter mobilization for the Democratic party.

Both the uniform and the social components of the new voter mobilization dynamics for the Democratic party can be included in the above model as

$$dD/dt = fR + bRD + mN + aND, \quad (2)$$

where N is the proportion of the electorate which is nonvoting, and m and a are parameters of the model. The parameters m and a, respectively, reflect the probability of mobilizing a Democratic supporter at an instant in time from within the localized pool of nonvoters due to the nonvoters' consideration of the appeal of the national campaign independent of local partisan traditions, as well as to the interaction between the nonvoting population and the local Democratic partisan environment.

Finally, it is possible that none of the components of the model describing fluctuations in Democratic partisan populations will completely capture the aggregate movements between Republicans and Democrats as well as between nonvoters and Democrats. This addresses the realistic considerations involved in mapping any model to a body of data. Thus it is important to include a constant term in the above expression that allows for Democratic population change independent of the included components. Equation 2 can now be rewritten as

$$dD/dt = fR + bRD + mN + aND + k, \quad (3)$$

or, for economy,

$$dD/dt = R(f + bD) + N(m + aD) + k, \quad (4)$$

where k is the constant element of the derivative and a parameter of the model.

It is important to note that while the analysis here refers to the above components of the model in terms of voters moving from one group to another, the components are actually capturing the net movements between groups. Thus, the parameter b captures the net interactively determined change between the Democratic and Republican parties. This "net" is, of course, the difference between the total of Republican-to-Democratic conversions and the simultaneous Democratic-to-Republican conversions. This is a characteristic of all models, both statistical and formal, which rely on aggregate-level data. There is no way for independent estimates to be derived that capture the changes in both directions.

However, the question remains as to whether this potential crisscrossing movement would seriously bias one-way interpretations that one might like to draw from the analysis. This depends on the degree of crisscrossing that actually occurred. The data used in this analysis clearly suggest that any potential interpretive bias would be extremely small. Using U.S. county-level aggregations, only eight (.3%) of the approximately three thousand counties experienced a decrease in the Democratic vote in 1932 combined with an increase in the Republican vote. If there had been a significant degree of crisscrossing, surely this would have been more detectable on the national level, ecological considerations notwithstanding. The situation is similar regarding Democratic and nonvoter crisscrossings in 1932. Only 2.4% of the counties experienced a decrease in the Democratic vote combined with an increase in the nonvoter populations. In 1936 there were larger number of counties experiencing decreases in the Democratic vote combined with increases in the Republican and nonvoting populations (34% and 22% respectively). However, *all* of these cases occurred in farm areas, which are accurately accounted for in the conditioning analysis presented below. Thus the one-way movements of the electorate clearly seem to dominate these data. Moreover, this closely corresponds to a historical reading of the politics of the times as it is presented in the vast related extant literature. Thus while a discussion in terms of net movements among parties and between parties and nonvoters more closely fits the technical realities of any model that utilizes aggregate-level data, interpretations that phrase these movements in one or another direction probably do no injustice to a historical analysis of the politics of these years.

The model describing longitudinal change in the Republican party population can be developed similarly to that for the Democratic party. Republican defections to the Democrats (or in some cases gains from the Democrats) will occur either uniformly across the nation, independently of the strength of the local Democratic presence, or through the process of local competition in which the strength of the Democratic traditions within each community will condition the rate of partisan change (again, the so-called uniform and social components of the model). Furthermore, gains or losses for the Republicans due to nonvoter volatility can be similarly directed. Given the weak economic conditions of 1932, some Republicans may have simply stopped voting, thus joining the ranks of the uninvolved. Yet some nonvoters with Republican tendencies may have felt that it was time to get involved when their favorite party was under attack. These potential gains and losses for the Republican party could have taken place uniformly throughout the United States, dependent only on the availability of nonvoters in a given community. Alternatively, the nonvoter-Republican shifts may have been dependent upon the interactive effects of the nonvoter and Republican populations. All such partisan and nonvoter trade-offs with regard to the Republican party can be captured with the statement

$$dR/dt = D(q - bR) + N(s + wR) + j, \qquad (5)$$

where q, b, s, w, and j are parameters of the model. Here, the parameters q and s control the "uniform" inputs to changing Republican support, whereas the parameters b and w govern the "social" inputs. Note the negative coefficient for the parameter b. This results from its appearance in the statement for change in Democratic support over time (i.e., equation 4). The negative coefficient for the parameter b maintains the accounting compatibility of both models and has implications for the procedures used to estimate the parameters. The parameter j is the constant term of the derivative and is included to identify Republican partisan change that cannot be captured by the other components of the model.

Changes in the nonvoter population of the United States are due to new voter movements to or from the Democratic and Republican parties. New voters can be carried toward a particular party on a wave of national excitement, again, independently of existing local partisan strengths. However, new voters also can be drawn toward a party due to the influence of the local party apparatus or led by the partisan directional cues sensed from the surrounding environment. These two paths address the uniform and social components of the models and can be incorporated directly in a model describing longitudinal fluctuations in the nonvoter population. Thus we have

$$dN/dt = D(g - aN) + R(v - wN) + y, \qquad (6)$$

where g, a, v, w, and y are parameters of the model and are identified with respect to the "uniform" and "social" components of the model as are the corresponding components in equations 4 and 5. The parameters a and w have occurred elsewhere and are included again here to preserve the population compatibilities of the overall system. The parameter y is the constant element of the derivative and describes change in the nonvoting population that cannot be isolated by the partisan-directed components of the model.

Equations 4–6 constitute an interdependent system of three nonlinear differential equations that together describe the mass electoral dynamics between the Democratic and Republican parties as well as between both parties and the nonvoting population. Throughout, the system includes both the "uniform" and the "social" components of population trade-offs between the three electoral groups (Democrats, Republicans, and nonvoters). The system has general properties and is entirely symmetric in tracing aggregate population shifts from any one group to all other groups. (Some characteristics of the above models written in reduced form are presented in Appendix A.)

While the system can be used to estimate the partisan and nonvoter trade-offs for the entire United States without modification, additional leverage can be gained by identifying different social conditions that could change the magnitude or direction of the partisan and nonvoter shifts. For example, we may be interested in how the system of electoral competition differed in urban areas as compared with rural areas. This can be done by conditioning the system with respect to a separate variable of interest—specifically, by writing each parameter as a linear function of this additional variable (referred to as the "conditioning" variable). Thus, using the parameter f as an example, we can rewrite the parameter using the form

$$f_0 + f_1X.$$

Writing all parameters in this fashion restructures the dynamic characteristics of the overall system with respect to the conditioning variable X (see also Jackson 1987).[3] The unconditioned parameters are recovered under the conditions where $X = 0$. The complete system is now

$$\begin{aligned} dD/dt = {} & R[(f_0 + f_1X) + (b_0 + b_1X)D] \\ & + N[(m_0 + m_1X) + (a_0 + a_1X)D] \\ & + (k_0 + k_1X) \end{aligned}$$

$$\begin{aligned} dR/dt = {} & D[(q_0 + q_1X) - (b_0 + b_1X)R] \\ & + N[(s_0 + s_1X) + (w_0 + w_1X)R] \\ & + (j_0 + j_1X) \end{aligned}$$

$$\begin{aligned} dN/dt = {} & D[(g_0 + g_1X) - (a_0 + a_1X)N] \\ & + R[(v_0 + v_1X) - (w_0 + w_1X)N] \\ & + (y_0 + y_1X). \end{aligned}$$

The Data and Estimation

Estimating the parameters in this system is a nontrivial problem. However, an extensive literature does exist that addresses such problems as well as an entire class of related issues. Standard approaches using regression techniques are of no utility here. It is not possible to solve for D, R, and N explicitly. Tuma and Hanna (1984), as well as Coleman (1981), offer linearizing techniques that are useful for analyzing simpler models. These techniques are pursued primarily to recover known statistical properties of the estimators but require algebraically approachable systems allowing the uncoupling of equations. Rather, the model must remain in derivative form and estimations must be obtained using numerical techniques to obtain longitudinal population trajectories for each group. In the social sciences, examples of the use of such techniques include estimations of systems of difference and differential equations addressing concepts of partisan competition (Brown 1987; Przeworski and Sprague 1986), as well as systems of equations modeling arms race competitions (Ward 1984). The procedures are commonly described in the engineering literature and are often used to solve practical problems employing systems of interdependent differential equations. A lucid summary of such techniques, as well as an introduction to the broad literature on the subject, can be found in Hamming 1971 as well as Dennis and Schnabel 1983. A more complete description of the techniques as they are used in this analysis is contained in Appendix B.

The data used to estimate this system are the electoral returns for all counties in the United States from 1928 to 1936.[4] There are approximately three thousand counties in the United States. All population measures for each group (Democrats, Republicans, and nonvoters) are written as proportions of the eligible electorate (21 years and older). Census information for all counties has been merged with the electoral data to produce an unusually complete collection of U.S. data for that time period.[5]

Three conditioning variables are used in this analysis. They are (1) urbanization (the total urban population measured as a proportion of the total population), (2) the level of farm activity, measured in terms of total county acreage under farm cultivation, and (3) the average number of wage earners in manufacturing industries during 1929, measured as a proportion of the total population. These three conditioning variables are defined as in the 1930 U.S. census, and are included here because their significance to the politics of the period has been repeatedly addressed (not always with parallel interpretations) by the topical literature (Clubb 1978, 75–79; Ladd and Hadley 1978, 66; Lubell 1965, 57; Petrocik 1981, 57; Sundquist 1983, 217).

While other conditioning variables could be used (and others were in analyses not reported here), these three variables seem of particular heuristic value in identifying major differences in the nature of the partisan competitions along lines around which there are prior expectations. For example, contemporary Democratic support is located in large part in urban settings. However, previous to the 1930s, Republicans depended heavily on urban support. How that transfer from Republican to Democratic urban strength took place, and under which dynamic settings is a question of importance substantively. For example, it is of interest to inquire whether the onset of the depression sparked a realignment of existing urban voters in 1932 or rather, whether it caused an upsurge in new voters to swell the Democratic ranks in such areas. Similarly, farmers were among the first groups to be affected by the depression. An examination of the dynamic characteristics of the partisan competitions in farm areas will address questions of whether large sectors of the farm-related population saw the 1932 and 1936 elections as times to abandon the Republican ship, or as times to board the Democratic movement from the quiet land of the previously uninvolved. The working populations fill a similar role in this analysis. Comparing the nature of the partisan competitions in both working and farm areas allows for an examination of crucial differences between diverse populations both in the timing and the manner of their movement to the Democrats. While both the working and farm populations were heavily affected by the depression, it is of interest to know whether differences with the economic, social, and political conditions for each group encouraged dissimilar partisan dynamics.

All of the conditioning variables are standardized to a mean of zero and a standard deviation of one before being entered into the analysis. The transformation inspirits the intuitive interpretation of the conditioning variables as measures of the social milieu, recognizing that small changes in a social environment (subjectively detectable as a shift in local ambience) can result in more dramatic electoral consequences (see also Blau 1977; Blum 1985; Simmel 1955).

The period from 1928 through 1936 is broken up into two separate time spans. Thus the model is evaluated based on its ability to explain longitudinal county-level population fluctuations for the Democrats, Republicans, and nonvoters for the period from 1928 through 1932 and then again from 1932 through 1936. We are, of course, anticipating that the model will detect and reveal underlying systemic patterns from within the data. However, the system evaluated here measures *change* between and among partisan and nonvoter populations. In situations in which little systemic change has occurred nationally, the model should perform poorly with regard to the measure of fit. In such a case, local noise would dominate. On the other hand, in those cases in which the model does discern a significant national trend in terms of the voter and nonvoter trade-offs, the measure of fit should reflect the system's success in characterizing the period's mass dynamics. Throughout all of the analysis, each case (i.e., county) is weighted by its eligible population.

This analysis examines the mass dynamics of the electoral movements during an unusual period of time in U.S. history. Since available survey data for this period are of questionable reliability, aggregate measures have been used extensively in the extant realignment literature in an attempt to unravel some of the electoral mysteries of those years. This analysis pursues an unusual treatment of the same type of data analyzed elsewhere. This analysis finds correspondence with results by Kramer and others that suggest that the analysis of dynamic aggregate data can be superior to cross-sectional survey data in determining individual-level behavior when the examined behavior involves a systematic longitudinal component (Kramer 1983; see also Barrilleaux 1986; Campbell 1986; Irwin and Lichtman 1976; Parent, Jillson, and Weber 1987; Powell 1986; Sprague 1976). The present analysis is formulated from the perspective of aggregate population trade-offs between groups. The expectations of the model rest with the direction of change in aggregate group memberships. Where aggregate relationships are strong, the corresponding expectations regarding individuals are buttressed from the perspective of the characterization of the mass dynamics.

Results

The results of the parameter estimations for the system are contained in Tables 2–5. The estimates are separated by time periods as well as by the conditioning variables used in the analysis. On the left sides of the tables are the estimates for the 1928–32 period. The estimates for 1932–36 are displayed on the right sides of the tables, next to the estimates for the earlier period. The fits of the models with regard to each group (Democrats, Republicans, and nonvoters) are also included in the tables. The chi-squared statistics test the statistical significance of each estimate in terms of its influence on the prediction surface generated by the overall system and are explained more thoroughly in Appendix B.

Substantive interpretations of the system's characterization of the mass dynamics of the period are difficult to obtain solely from an examination of Tables 2–5. However, while the parameter estimates are used to produce a graphical analysis of the overall system, an interpretation of a few of the estimates is useful heuristically as an introduction to how the graphs are produced. Note in Table 2 that the estimates for the parameter *b* between both periods (in the unconditioned case) change dramatically in magnitude. Recall

Table 2. Unconditioned Parameter Estimates

Parameters	1928–32 Estimates	1928–32 Chi-Squared (*df* = 2)	1932–36 Estimates	1932–36 Chi-Squared (*df* = 2)
F	.17663	552,344	.16550	261,216
B	.28495	210,164	.00220	15
M	.05874	202,611	.04801	78,357
A	.21841	218,718	.08603	44,440
G	.06158	39,570	–.07332	115,710
Q	–.04302	34,852	.05017	94,374
S	–.02308	32,020	–.00871	4,170
W	–.18302	154,735	.04190	4,571
V	–.11234	129,542	–.14990	200,499
K	–.00918	22,113	–.00349	2,369
J	–.01609	70,537	–.00461	6,737
Y	–.02275	79,659	–.00806	11,773

Note: 1928–32 models and fits: Republican, .72210; Democratic, .70980; nonvoter, .29513. 1932–36 models and fits: Republican, .10401; Democratic, .46233; nonvoter, .59062.

Table 3. Urban-conditioned Parameter Estimates

Parameters	1928–32 Estimates	1928–32 Chi-Squared (*df* = 2)	1932–36 Estimates	1932–36 Chi-Squared (*df* = 2)
F1	–.041184	73,638.4	.016983	6,436.4
B1	–.013620	1,035.9	.007756	447.3
M1	.006823	6,293.5	.017480	19,756.1
A1	.003570	154.7	.009544	1,354.1
G1	.012993	4,157.1	–.013117	9,240.0
Q1	.045386	89,041.7	–.018512	31,892.4
S1	–.014611	27,723.4	.007009	5,468.0
W1	.008769	949.4	.001171	10.6
V1	.009201	2,043.3	–.005351	638.6
K1	–.001442	1,323.4	.000179	13.5
J1	–.000850	445.5	–.000590	254.3
Y1	–.003496	4,321.9	–.001291	699.7

Note: 1928–32 models and fits: Republican, .75175; Democratic, .73101; nonvoter, .30703. 1932–36 models and fits: Republican, .16159; Democratic, .533219; nonvoter, .61885.

Table 4. Workers-conditioned Parameter Estimates

	1928–32		1932–36	
Parameters	Estimates	Chi-Squared (*df* = 2)	Estimates	Chi-Squared (*df* = 2)
F1	–.056730	50,843.8	.018393	2,814.2
B1	–.025263	1,153.9	.008776	197.6
M1	.004884	994.3	.025507	13,335.3
A1	–.003974	57.8	.011860	611.7
G1	–.007105	380.4	–.014155	3,175.2
Q1	.081014	82,174.5	–.030970	26,567.4
S1	–.018781	13,699.8	.010886	4,112.4
W1	–.034929	5,483.0	.002252	14.8
V1	.030527	8,471.5	–.007042	402.6
K1	–.002115	908.5	.000393	21.1
J1	–.001122	241.1	–.000650	98.1
Y1	–.003614	1,518.3	–.001862	460.6

Note: 1928–32 models and fits: Republican, .76030; Democratic, .74926; nonvoter, .32356. 1932–36 models and fits: Republican, .17262; Democratic, .52578; nonvoter, .61129.

Table 5. Farm Density–conditioned Parameter Estimates

	1928–32		1932–36	
Parameters	Estimates	Chi-Squared (*df* = 2)	Estimates	Chi-Squared (*df* = 2)
F1	.049522	5,045.8	–.055609	4,333.1
B1	.035395	256.4	–.022492	183.7
M1	–.003735	259.9	–.012324	1,572.3
A1	.012268	227.9	–.013577	392.4
G1	–.039536	3,149.1	.031656	4,860.7
Q1	–.074097	20,940.7	.032871	9,013.1
S1	.017093	5,855.5	–.019620	6,467.1
W1	–.032939	1,168.3	–.002274	3.6
V1	.038948	1,789.7	.018547	447.5
K1	.001887	215.5	–.003577	602.0
J1	–.000359	8.5	.001869	265.2
Y1	.003798	506.7	.001154	58.7

Note: 1928–32 models and fits: Republican, .78489; Democratic, .73126; nonvoter, .31185. 1932–36 models and fits: Republican, .15693; Democratic, .54827; nonvoter, .63657.

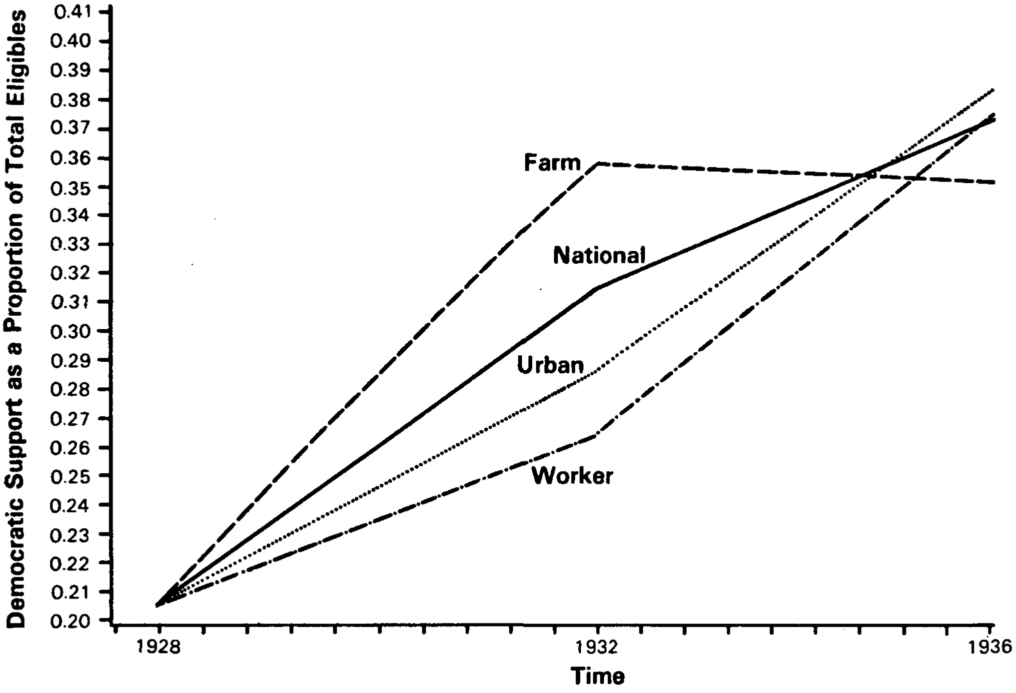
that the parameter *b* represents gains for the Democrats due to interactive losses (i.e., the "social" component of the model) from the Republicans. These results suggest that the Democrats gained heavily from the Republicans in this fashion during the 1928–32 period but not nearly so heavily during the later period. Similarly, the estimate for the parameter *q* changes substantially from the early to the later period. In this case, the magnitude remains approximately the same, but the sign changes. Again, the parameter *q* represents the "uniform" component of the model describing aggregate group change from the Democrats to the Republicans. These results suggest that Republican "uniform" losses did correspond with Democratic gains in 1932 but not in 1936. Indeed, this one parameter value (examined in isolation from the remainder of the system) seems to indicate that there were some uniform gains for the Republicans from the Democrats in 1936. (The graphic analysis below will show that some reverse movement did in fact occur in farm areas in 1936.)

An examination of the fits for the unconditioned cases similarly provides some useful initial interpretive guidelines with regard to the entire system. For the period from 1928 to 1932, note that the models account for a considerable amount of variance in the longitudinal population fluctuations for the Republican and Democratic parties. Note also that the model that characterizes change in the nonvoting population does less well during that time period. Compare these results with those obtained using the unconditioned system for the period from 1932 to 1936. While the model describing change for the Democratic party continues to account for a reduced but still substantial degree of variance in the aggregate Democratic outcomes, the model for Republican change now performs poorly. On the other hand, the model for change in the nonvoting population does very well. These initial results can only offer very tentative guidelines as to the interpretations of large-scale aggregate movements. However, they do suggest that during the 1928–32 period, the greatest aggregate movement on the county level was between Republicans and Democrats, as the model for nonvoter change encounters relatively less national trend and more local "noise" for that period. But in the 1932–36 period, the situation seems to have reversed. These results suggest that it is activity between the Democratic and nonvoter population groups that dominates the later period.

A more comprehensive description of the characteristics of the estimated system under the influence of the various conditioning variables can be obtained using a graphic analysis. The simplest form of such an analysis is a straightforward longitudinal plot of support for a particular party. Figure 1 presents such a plot for the Democratic party. There are four model-generated time paths presented in Figure 1. The path labeled "National" is the longitudinal trajectory for the Democratic party using the unconditioned estimates found in Table 2. This path is included in Figures 2 and 3 as well and serves as a base line from which to evaluate the other trajectories. The time paths labeled "Farm," "Urban," and "Worker" represent the system's predicted level of voter support for the Democratic party in areas that can be described as primarily farm-oriented, totally urban, and heavily working class in character.

Note that from 1928 through 1936, the Democratic party's strength continued to grow nationally (i.e., as seen using the trajectory labeled "National"). However, the greatest growth for the Democratic party between 1928 and 1932 was in the farm areas of the country and there seems to have been no additional growth in Democratic support from such areas after Roosevelt's initial election. However, Democratic support coming from heavily

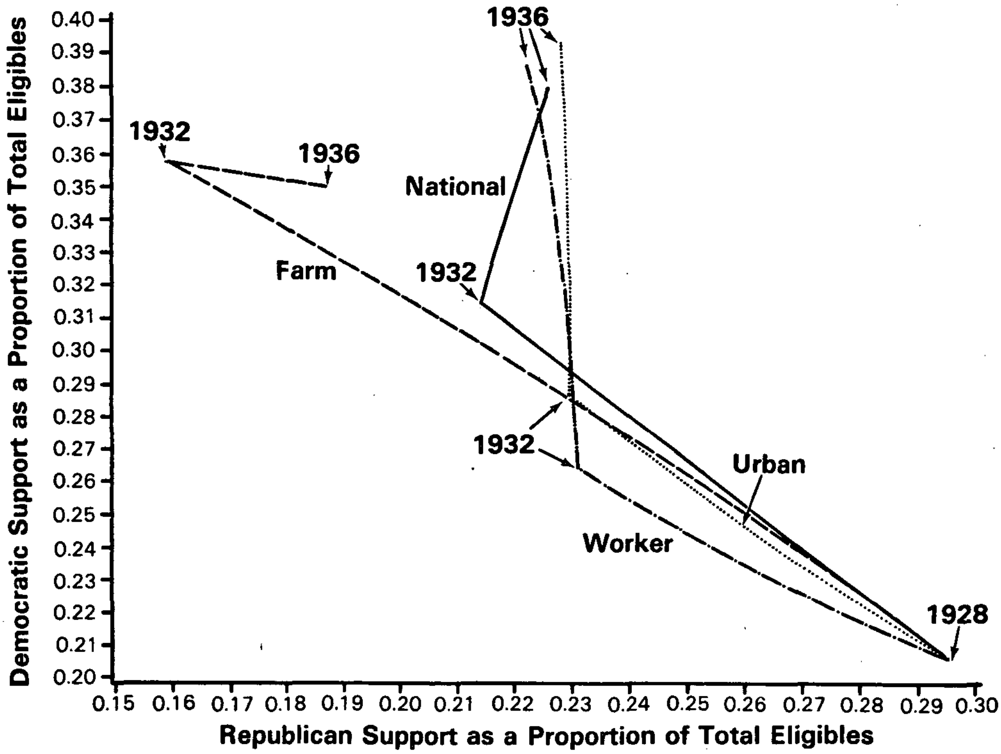
Figure 1. Democratic Support from 1928 to 1936



urban and "working class" areas exhibits an opposite pattern. Between 1928 and 1932, such areas contributed less to Roosevelt's first victory than did many of the other sections of the nation. Indeed, areas with high concentrations of workers produced the smallest growth in Democratic support in 1932. These results are of interest considering the focus of much of the realignment literature for the U.S. on the urban and working class basis of the Roosevelt coalition (see Degler 1971, 141; Petrocik 1981, 53). However, note that after the 1932 election, Democratic support from urban and working class areas increased dramatically, marginally surpassing the overall national strength of the party and completely eclipsing the now-stagnant growth from the farm areas. These results for 1936 seem to support arguments by Sundquist and some others (Sundquist 1983, 218–19; see also Lubell 1965, 57–63) that the urban and working class elements of the Roosevelt coalition did not emerge in their full form until after 1932. However, the trajectories in Figure 1 suggest surprising differences in the manner and timing in which various groups turned to the Democrats. In particular, the magnitude of the differences between the farm areas as compared with the urban and working class areas for 1932 is quite striking given the claims by some authors that the growth in Democratic support in 1932 was primarily affected by a broadly based sense of dissatisfaction (cutting across all groups) with the previous Republican leadership (Ladd and Hadley 1978, 87). Moreover, the dramatic reversal of these patterns after 1936 is noteworthy in terms of its magnitude and deserves further explanation.

A problem with Figure 1 is that it does

Figure 2. Democratic and Republican Trade-offs, 1928 to 1936



not allow for an examination of the relative trade-offs between groups; that is, it is not clear whether the growth in Democratic support coincides with an increase in new voter strengths or a decrease in local Republican fortunes. These trade-offs can be seen more clearly in an analysis of a type of graph called a "phase diagram." Figure 2 is a phase diagram representing change in Democratic and Republican support from 1928 through 1936. As with the other phase diagram described below, each curve on the plot represents simultaneous change in two separate populations. In Figure 2 the horizontal axis identifies Republican support from the pool of eligibles while the vertical axis represents Democratic support.

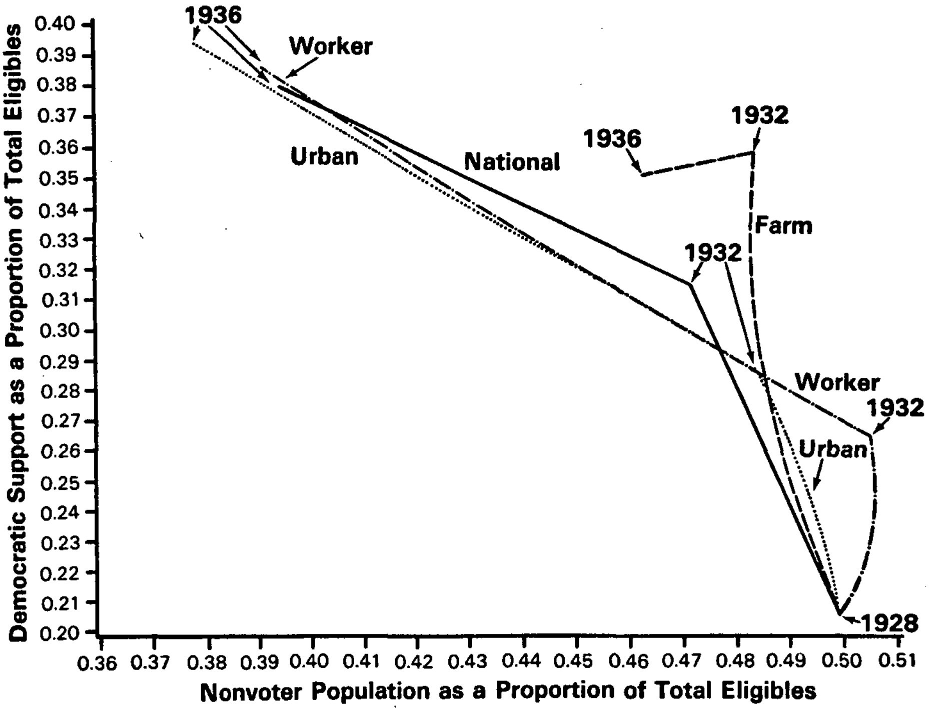
Note that each curve on the plot is labeled with respect to whether it is an unconditioned ("national") or a conditioned ("farm," "urban," or "worker") trajectory. Also note that each curve on the figure is labeled with regard to the three election years 1928, 1932, and 1936. As one visually follows any one of the curves (all beginning in 1928 and originating in the lower-right-hand corner of the figure) upwards and to the left, it is possible to examine the voter trade-offs between the two groups over time. For example, follow the curve labeled "National" diagonally up from its starting point in 1928 until its sharp bend upward in 1932. This part of the curve indicates that from 1928 to 1932 there was a large drop in national Republican support combined with an approximately equal gain in Democratic support, all computed using the entire estimated system (thus simultaneously controlling for all partisan-nonvoter shifts as well). Continuing up the curve from 1932 to 1936, there appears to be very little movement in Republican support (shown by the near vertical nature of the curve after 1932) and a substantial increase in national Democratic support. When a curve cuts a dramatic diagonal across the surface of a plot, this indicates a substantial simultaneous movement between the two populations represented on the figure's axes. When a curve is placed horizontally or vertically in the figure (or nearly so, relative to the other curves), this indicates little or no aggregate movement between the two populations (i.e., one population stayed constant while the other either increased or decreased).

While the curve for the national-level trade-offs is included in all phase diagrams for reference, the other curves tell the more intersting story. In Figure 2, note that the curve representing the system of competition as it occurred in predominantly farming areas marks a very dramatic diagonal sweep across the surface of the plot from 1928 to 1932. This suggests that a great deal of formerly Republican support coming from farm areas in the United States abandoned the Republican party and joined the ranks of the Democratic voters. Moreover, the magnitude of the Republican loss in farming areas, combined with that of the Democratic gain, surpasses those for urban or working class areas as well as for the national average. Such a strong movement away from the Republicans and toward the Democrats in farming areas probably had its origins in historical conditions reaching back more than 10 years. Recall that the La Follette movement in 1924 registered marked discontent with Republican farm policies (Sundquist 1983, 182–91) and that Calvin Coolidge did once say, "Well, farmers never have made money. I don't believe we can do much about it" (White 1965, 344). It seems that the onset of the Great Depression was the last straw for many farmers. While the Republicans may have maintained a substantial degree of farmer support throughout the 1930s, they do seem to have lost a good part of their earlier support in 1932. However, note that the Republicans did bounce back somewhat in the farm areas after 1932. This suggests that many of the switching farmland folks of 1932 stayed Democrats in 1936, but a few returned to their Republican moorings.

The pattern between the Democrats and the Republicans in farm areas differs remarkably from that in urban and working class areas. Figure 2 suggests that among urbanites there was a substantial Republican loss combined with a large Democratic gain primarily in 1932. However, in areas in which there were large numbers of workers, Republican support dropped between 1928 and 1932 with less than an equivalent Democratic gain. After 1932, Democrats seemed to have scored heavily from such areas without a concomitant Republican loss of comparable magnitude. This raises the question of what these working class voters did in 1932. While they seemed to have abandoned the Republican party in large numbers in 1932, they do not seem to have been as fundamental a component of the Democratic gains until after 1932.

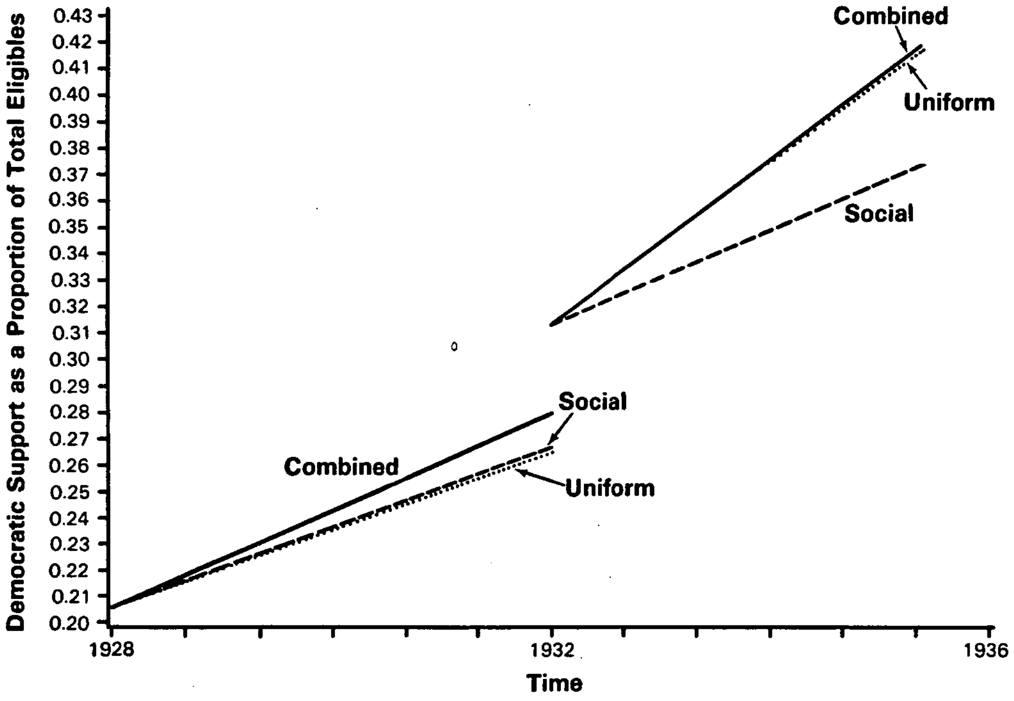
This puzzle regarding working class trade-offs between the Democrats and the Republicans is answered in large part with Figure 3. Figure 3 contains the longitudinal trade-off population trajectories for the two groups, Democrats and nonvoters. The horizontal axis represents the nonvoting population, measured as a proportion of the total electorate; whereas the vertical axis represents support for the Democrats (as in Figure 2). Note that the curve labeled "Worker" moves upwards and somewhat to the *right* after its beginning in 1928. From 1932 to 1936, the curve makes a marked change and moves diagonally upwards and to the left. These

Figure 3. Democratic and Nonvoter Trade-Offs, 1928 to 1936



results suggest that voters in heavily working class areas experienced no increase in mobilization in 1932 (and perhaps a degree of demobilization). This result, interesting in terms of the magnitude of the later shift, is not completely without interpretation guided by some of the extant realignment literature of the period. Recall that the 1932 election was not a radical departure from other elections in the 1920s with respect to Roosevelt's campaign rhetoric (Ladd and Hadley 1978, 38; Lubell 1965; Sundquist 1983, 208–10). Indeed, it was not until Roosevelt came to power that the shape of the future was so clearly directed in New Deal terms (Petrocik 1981, 53–54). While the depression made the Republicans an unpopular party in 1932, the Democrats were not clearly identified as a party of economic and social salvation (Key 1964, 523–24). The results of Figure 3 suggest that workers did not initially rise up and attempt to throw the Republicans out of the White House, regardless of whether or not they blamed that party for their own depression-related economic misfortunes. Rather, they remained inert, and some may have even stopped voting, withdrawing from political participation just as they had begun to withdraw from their participation in the national economy. This may have been tied to the dramatic decrease in the unionized work force at the time. They may have been struck by anger at their plight, but all were not motivated (or perhaps organized) to register their anger at the polling booths. One can only suspect that they did not really know where to turn for assistance

Figure 4. Simon Bounds for Democrats from Republicans (Urban-conditioned)



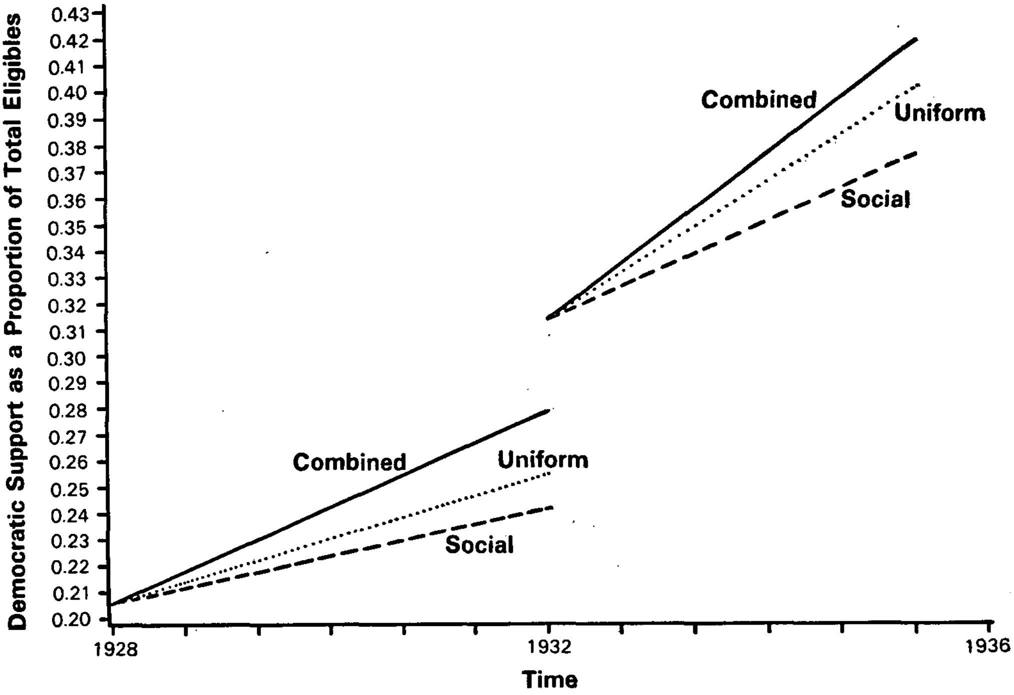
in 1932. They were conceivably more interested in their own domestic situations than in politics, and they were perhaps unsure of what any government could do for them, given decades of Republican political dominance and a previously widespread public acceptance of the principle of nonintervention in the private economy. But in 1936 they saw the difference between the parties, and partisan politics in the United States experienced a massive infusion of new as well as formerly demobilized voters from working class areas.[6]

Figure 3 suggests a related story for urban voters (of whom many were workers). There was little overall movement to the Democrats from the pool of nonvoters in urban areas between 1928 and 1932, as is indicated by the near vertical course of the curve labeled "Urban" through 1932 (although there was certainly variation between many particular urban areas, of course). Yet after 1932 the diagonal movement for the curve indicates that the urban areas experienced heavy Democratic mobilization among new voters. Voters in farm areas acted in an entirely different fashion. The Democrats managed to mobilize some new voters in 1932 from such areas. However, in 1936 the Democrats had no comparable success from the farming areas. Indeed, a fraction of their 1932 farmland supporters failed to turn out at all.[7]

The Relative Impact of the Mechanisms for Change

Figures 4–7 change the focus of this analysis to ask a question based on the

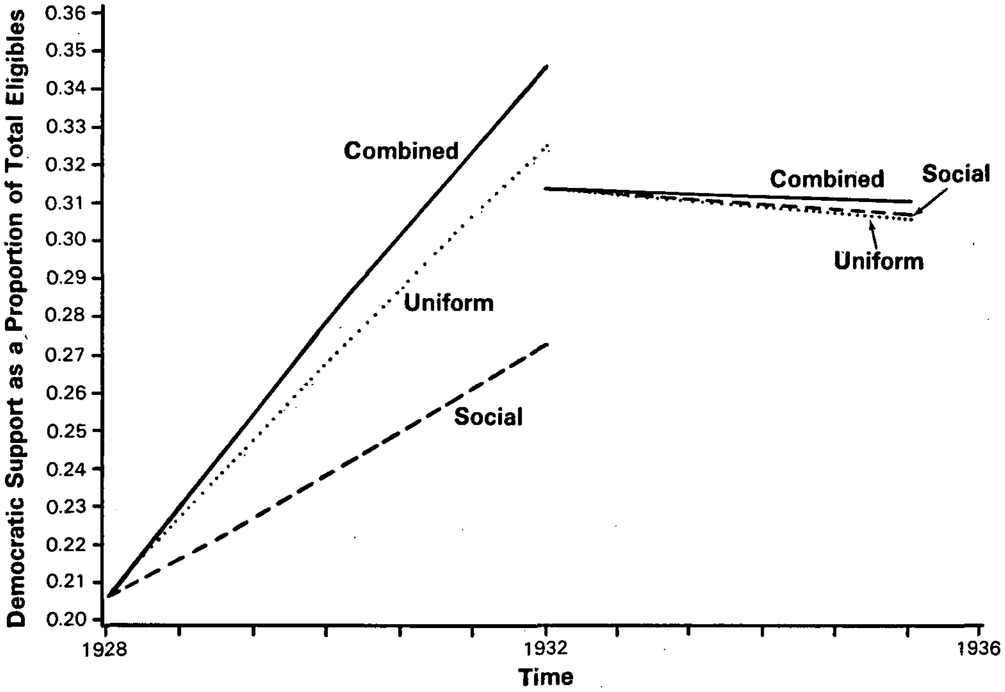
Figure 5. Simon Bounds for Democrats from New Voters (Urban-conditioned)



structure of the model explored here. Within each mathematical statement characterizing growth and decay for the partisan and nonvoter populations, there are both uniform and social components. It is descriptively useful as well as analytically important to evaluate the relative impact of the two components. Figure 4 displays what is titled the "Simon bounds" for aggregate partisan change *for* the Democrats *from* the ranks of the Republicans. Figure 5 contains a similar representation for partisan change for the Democrats from new voters. Both Figures 4 and 5 are drawn with respect to the urban-conditioned environment. Figures 6 and 7 are a similar representation for the farm-conditioned environment. The term *Simon bounds* references an early work by Herbert A. Simon (1957), in which substantive meanings were explicitly tied to formal mathematical expressions of social change of the type used here. In all figures, time is on the horizontal axis and Democratic support as a proportion of the total eligibles is on the vertical axis.

To begin with an interpretation of Figure 4, the curves labeled "Combined" represent the total longitudinal gains for the Democrats in urban areas and are identical to the urban-conditioned trajectories presented under the "Urban" label in Figure 1. The lines labeled "Uniform" and "Social" are computed using only the system's uniform or social components (respectively) as inputs to the joint Democratic and Republican totals. For example, to compute the Democratic vote represented by the lines labeled "Uniform" in Figure 4, the parameter b was set to zero.

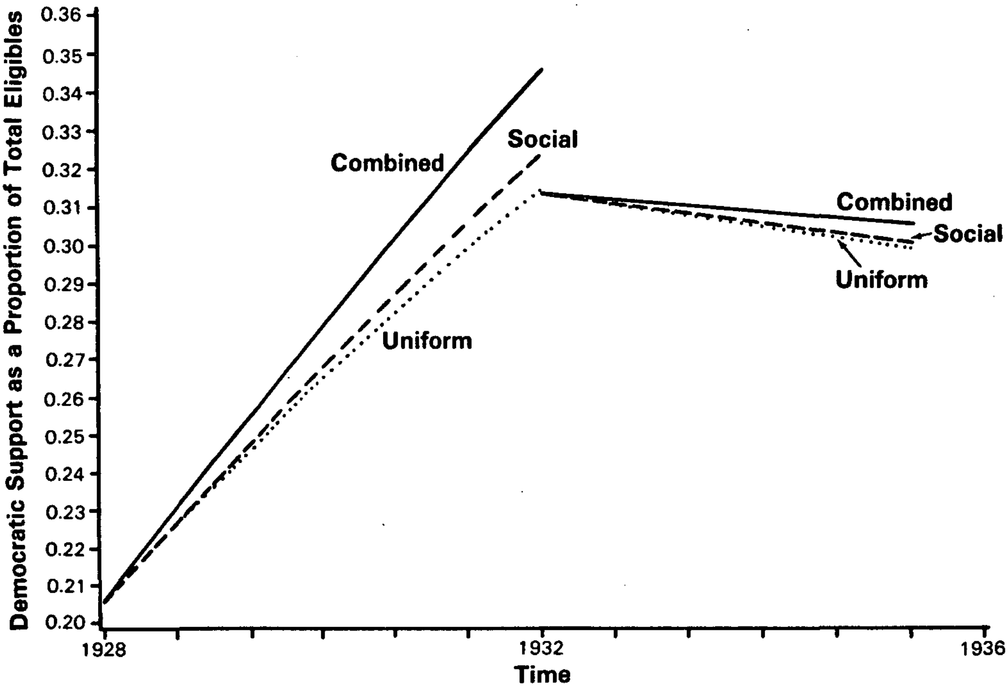
Figure 6. Simon Bounds for Democrats from Republicans (Farm-conditioned)



This parameter represents the probability of recruiting a Republican for the Democrats at an instant in time due to the interactive influences of the two existing partisan populations. Setting this component to zero leaves only Democratic gains due to nationally uniform Republican defections (i.e., parameters f and q, mediating the uniform components of the expressions for Democratic and Republican change). Alternatively, the lines labeled "Social" are computed by setting the uniform components between the Republicans and the Democrats to zero, leaving only the social component in the model. Briefly, the lines labeled "Uniform" in Figure 4 use only the uniform components of the model as inputs to the nationally changing partisan totals, whereas lines labeled "Social" use only the social components.

For urban environments during the period from 1928 to 1932 in Figure 4, note that the social component of the model contributes only a very slightly larger share of the national Democratic vote gain than the uniform component. This is indicated by the height of the social time path relative to the height of the uniform time path. Both the uniform and the social paths are lower than the combined path since the former both contributed to the national totals, and the absence of either would produce a lower outcome. This suggests that Democratic gains from the Republicans in 1932 in urban areas were approximately equally dependent on the localized abilities of the Democratic party to compete (i.e., converting more Republicans where the Democrats are strong and fewer Republicans where the Democrats are weak) as on a more uniformly

Figure 7. Simon Bounds for Democrats from New Voters (Farm-conditioned)



distributed sense of dissatisfaction with the Republicans.

The time paths on the right of Figure 4 (for the period from 1932 to 1936) indicate that the uniform component was dominantly responsible for any further Democratic gains from the Republicans in urban areas. This is evidenced by the much greater height of the uniform time path relative to the social time path. Recall from Figure 2 that there were very few Democratic gains from the Republicans in urban areas in 1936. The results of Figure 4 suggest that the Democrats did not have to rely on their own localized partisan strengths in order to maintain those gains.

Figure 5 contains the Democratic time paths identifying the "Simon bounds" with respect to Democratic gains in urban areas from the new voters. Comparing the 1928–32 with the 1932–36 period, note that the relationship between the uniform and social time paths is similar between periods. In both periods, Democratic gains from the new voters were relatively independent of local Democratic party strengths. Apparently these new voters (fewer in 1932 than in 1936) were driven by the national sense of crisis and (especially in 1936) the overall Democratic appeal. In other words, Democratic mobilization of new voters in urban areas was relatively less dependent on existing local Democratic partisan strengths.

Recall from the discussion of Figures 2 and 3 that Democrats gained a great deal of Republican support as well as a substantial amount of new voter support from farm areas in 1932. Figures 6 and 7 present the Simon bounds for Democratic gains from the Republicans and the new voters within farm-conditioned environments. The trajectories on the left side of Figure 6 indicate that the Republican-to-Democrat defections in 1932 were not a result of locally dependent partisan competitions. This is evidenced by the much greater height of the uniform trajectory relative to the social trajectory. Following 1932, there was much less of a difference between the separate components of the competition. But the Democrats did not gain much from the farm-based Republicans in 1936 (in fact, they seem to have lost support there).

Figure 7 reveals an interesting difference from the pattern presented in Figure 6. From the new voters in 1932, the dominant component of the Democratic gains came from the local partisan competitions in areas where existing Democratic organizations could successfully mobilize former nonparticipants. In this figure, the social component has a substantially higher trajectory than the uniform component. The same pattern exists after 1932, but the relative impact of farm-based new voters on Democratic gains in 1936 was slight overall (from Figure 3).

In summary, Figures 4–7 present an interesting and contrasting picture of the dynamics of partisan competitions in urban and farm environments. In the urban areas, the movement toward the Democrats by new voters was a phenomenon that occurred in large part uniformly throughout these areas, relatively independent of existing Democratic strengths. Among Republican-to-Democratic conversions in 1932, the effects of the uniform and social components were more or less equal. In farm areas the situation was also complex. Uniform movement dominated among Republican-to-Democratic conversions. However, new voters from farm areas were most easily brought into the Democratic ranks when the local Democratic presence was relatively strong.

The nature of this recruitment as described above corresponds to findings reported elsewhere by Beck (1974) regarding the environmental properties of county-level partisan competitions during periods of dealignment. In farm areas, the early Democratic success came from former Republicans across-the-board (i.e., "uniformly") who were fed up with Republican farm policies (or perhaps the lack of them) and some new voters in locations in which there was an existing Democratic presence. Thus Roosevelt's relatively "gentle" campaign, filled with farm-oriented themes, aided local Democratic mobilization efforts among new voters in rural areas. But the already-mobilized former Republicans did not need the party contact. They jumped ship more uniformly than the new voters, and their impact on the early Democratic success was much greater than that of the new voters.

It was only after 1932 that the New Deal coalition began to form around identifiable centers of Democratic strength in urban areas. When this occurred, the movement was relatively less dependent on local partisan competitive abilities than on the national momentum of the times. The campaign of 1936 did not represent the politics of the former status quo. The Democrats had a new and coherent message, a message that had direct appeal to the urban masses. It seems that the radical nature of the message transformed the campaign from one where local party organizations were a major factor in the processes of converting Republicans (as in 1932 for urban areas) to one in which the party more accurately benefited from the windfall resulting from the newly energized and volatile electorate.

These results for farm and urban areas suggest that during periods of rapid change in mass voting patterns party organizations are more likely to be victims or beneficiaries, than causes, of the mass movements. When the movements are of smaller magnitude, the activity of the party organizations have much greater impact. However, large aggregate movements, especially involving new voter activity as with the Democrats in 1936, could also be seen as the beginning of the renaissance of local party apparatuses, reenergized by the masses after years of decay with little national-level guidance. When partisan politics settled down, the enhanced organizational strength of the Democratic party was certainly a factor in the dominance of the party in national politics until the 1950s.

Remarks

In this study I develop and explore a model of mass political behavior with respect to the realigning period in the United States from 1928 to 1936. The analysis suggests that vote switching from the Republicans to the Democrats dominated the 1932 part of the realignment. However, discontent with the Republicans was not distributed equally or even near equally across social groups in the nation. Voters in farm areas—both former Republicans and some new voters—joined the Democratic ranks in large numbers. But urbanites responded to the Democratic appeal with less enthusiasm in 1932. Republican urban voters converted at a rate lower than that for the nation as a whole, and the nonvoters in these areas generally stayed put. Moreover, these results suggest that some workers may have even withdrawn from active participation in the electorate in 1932 rather than be drawn toward an emergent Democratic party that had not yet developed a clear identity.

The election of 1936 changed the character of Democratic politics. Some voters from the farm areas returned to the Republican fold. But urbanites and workers flooded the Democratic party's ranks. Moreover, these new Democrats of 1936 were also predominantly new voters.

The model developed here characterizes the nature of the partisan competitions as a dynamic process containing both nationally uniform and locally interactive partisan ingredients (the so-called uniform and social components of the model). In a situation of national crisis combined with large-scale mass movements, the uniform components often dominate, especially given weaknesses in the local party structures of at least one party. But when the mass movements are less severe but the political climate is favorable, the social components of partisan mobilization can yield substantial gains as well. In either case, the rejuvenation of the party organization as linked to the resurgence in its national fortunes must certainly enhance the party's long-term prospects of securing the attachments of these newly oriented partisans.

Appendix A

All of the models (i.e., the entire system) can be rewritten in reduced form. For purposes of explanation (since all three derivatives have similar structural forms), the present discussion is limited to the mathematical statement for dD/dt. Taking advantage of the identity $D + R + N = 1$, solving for N and making the substitution into equation 3 produces

$$dD/dt = fR + bRD + m(1 - R - d) + aD(1 - R - D) + k.$$

This simplifies to

$$dD/dt = (k + m) + D[(a - m) + (b - a)R - aD] + (f - m)R,$$

or, in reduced form,

$$dD/dt = \beta_0 + D[\beta_1 + \beta_2 R - \beta_3 D] + \beta_4 R. \qquad \text{(A-1)}$$

All of the right-hand-side components of equation A-1 are standard features of many such reduced form models, which are described in the literature on dynamic modeling (Danby 1985; Haberman 1977; Hirsch and Smale 1974; Huckfeldt, Kohfeld, and Likens 1982; Luenberger 1979; Nisbet and Gurney 1982). In reduced form (thus in the complete absence of the original structural equations), substantive interpretations might be proposed as follows: β_0 represents a term characterizing constant growth; β_1 describes growth based only on existent levels of D (exponential growth or decay); β_2 controls the interactive input to the model as either growth or decay; β_3 is a logistic limitation to growth, where growth in D slows as D approaches an upper bound; and β_4 represents noninteractive gain (or loss) dependent on existent levels for R. The problem with the reduced form expressions is that the above interpretations do not characterize the more complex social processes, such as are revealed through the imbedded (i.e., nonreduced) parameters found in the original structural equations. This problem is characteristic of all reduced form models, which leads to the natural desire to estimate complex systems in their structural representation. Hanushek and Jackson offer the following interpretation of the two forms: "The reduced form equations summarize the entire structural model in terms of the total changes expected in each endogenous variable from a change in any one of the exogenous variables. The structural model on the other hand 'explains' how those changes occur and describes the behavioral process underlying the predicted changes" (1977, 227).

It is not necessary to estimate the models in their reduced form. Indeed, for estimation purposes, the models are best left in their original structural form (with all of the model-to-model parameter interdependencies left explicit), thus allowing for statistical tests for all of the original (nonreduced) parameters. This is a general problem of nonlinear parameter estimation toward which is directed a sizable body of literature in both the engineering and econometric fields (Bard 1974; Dennis and Schnabel 1983; Hamming 1971; Judge et al. 1982, 773–74; SAS Institute 1984, chap. 21). The precise methods of parameter estimation as they are used in this analysis are explained in Appendix B.

Appendix B: The Calculations

The iterative parameter estimation procedure used in this analysis has its roots in a broad literature spanning a variety of diverse disciplines. Perhaps the most lucid description of the general procedure can be found in Hamming 1971. The focus of the related literature is the merger of standard numerical approximation techniques with least squares unconstrained optimization procedures. Much of the relevant literature is identified in Dennis and Schnabel 1983 (pp. 364–70; see also Hamming 1973). Interested readers might also find useful Fletcher 1965, Powell 1964, and Dennis, Gay, and Welsh 1981a, 1981b.

The following description of the estimation technique used in this analysis is necessarily brief. A more thorough description of the technique (including software written in SAS) is available from me.

The estimation process begins by computing a trajectory using guessed (but plausible) values of the parameters (a process to be repeated often with different values). A Runge-Kutta approximation to definite integration is then used, the first step of which takes the form

$$\mathrm{DNEXT} = D + h(dD/dt),$$

to compute an overtime trajectory for each partisan population, where h is a small number. This iteration is repeated a fixed number of times to yield the next election's predicted partisan total (in the above example, for the Democrats).

The predicted values for the Democrats, Republicans, and nonvoters are then evaluated with regard to their fit to the actual data. The fit is calculated as

$$FIT = 1 - (RSS/TSS),$$

where *RSS* is the residual sums of squares between the predicted and actual values for each group, and *TSS* is the total sums of squares measuring the total variation for each group between the two elections.

The partial derivatives of the fit surface with respect to all parameters are then computed as

(change in fit / change in parameter).

This is accomplished by disturbing each parameter around its present value and evaluating the concomitant change in fit. All of this is done for one parameter at a time.

The estimated parameter values are then moved through the parameter space in order to maximize the fit based on the directional information contained in the vector of partials for the fit surface. This movement is accomplished iteratively using the relation

$$BNEXT = B + z(P),$$

where the vector of partials for the fit surface is *P*, the vector of parameter values is *B*, and *z* is a small number. The value of *z* can vary depending on the steepness of the overall fit surface and the proximity of the maximum to that surface. Movement in the parameter space continues until the maximum to the surface has been reached, as is indicated by an evaluation of the partials to the fit surface.

With any one attempt at arriving at the maximum of the fit surface, there is no guarantee that the achieved maximum is the global maximum of the surface. This is characteristic of all nonsmooth problems of this sort. The usual practice of varying the initial parameter values as a safeguard has been followed here.

The chi-squared statistics for the parameters test the impact of each parameter on the predicted values for each group (i.e., Democrats, Republicans, and nonvoters) from which the measures of fit are derived. First, predicted values for each group are calculated using the optimal values for each parameter. Second, new predicted values are calculated after setting each parameter to zero (one at a time). The differences between the two population sets are used to compute the chi-squared statistics. Thus a low chi-squared value indicates that an estimate has little impact on the predicted surface (and thus the model) and that the null hypothesis that the parameter equals zero should not be rejected.

Interested readers can obtain a free copy of the computer program which was used for the above estimations by contacting the author at the BITNET address POLSCB at EMUVM1. The program is machine-readable and thoroughly annotated. All inquiries should include a BITNET address to which the response should be sent.

Notes

1. There are three basic reasons for the previous unavailability of a usable complete collection of U.S. county-level aggregate data. First, the electoral data are available from the Inter-University Consortium for Political and Social Research data collections in what amounts to a scattering of over two hundred separate data files. County-level returns for all states and all years have not been organized into a few accessible, large, data files. Second, the available data sets, although scattered, are still terribly complete. There are data for literally hundreds of parties, covering all U.S. congressional, presidential, and many state elections. The bottom line is that the variable names in the separate data sets are not comparable. One cannot simply merge the data sets since the variable names do not correspond in year or party. Third, the information that is necessary to merge the various data sets is contained in the variable labels. There are often hundreds of variable labels for the many data sets, each of which needs to be read individually, and from which new

variable names need to be constructed. Thus the process of identifying particular partisan returns for all counties in the U.S. has been unusually difficult, given normal constraints of staff, budget, and personal wear and tear.

2. Since this analysis focuses on the time period from 1928 to 1936, some questions regarding some of the long-term aspects of the realignment remain unanswered. It is not clear whether a vote-switching realignment in 1932 implies a permanent reorientation of what would be thought of as the "normal vote" for many voters. Some have suggested that many of the switchers of 1932 returned to their earlier Republican habits by 1940 (Petrocik 1981, 57). Survey responses (were they available) could have been used to query the voters' inner psychological commitments to particular partisan labels and attachments. However, in the absence of such data, reliance is made on an examination of the voting patterns of the early period, identifying vote switching and nonvoter interactions but leaving open the question of whether or not both the 1932 and the 1936 elections represented a deviating electoral period for some voters in which previous partisan allegiances were subsequently reestablished.

3. Writing each parameter in this fashion is different from simply estimating the nonconditioned system separately, using select subsets of the aggregate data for each estimation. Writing the parameters as a linear function of a conditioning variable allows the use of all of the data, as well as an interpretation of the system based on progressive changes in characteristics of the social environment.

4. The data utilized here were made available by the Inter-University Consortium for Political and Social Research. Neither the original collectors of the data nor the consortium bear any responsibility for the analyses or interpretations presented here.

5. The problem of organizing the data described in n. 1 was accomplished by first printing (on magnetic disk) a listing of the many data sets. A Basic program was then constructed which "read" each of the listings. This program also wrote computer code in SAS based on the information from these listings. The result was a program written in SAS that recoded all variables in all data sets to have common names containing embedded information that had been extracted from the original variable labels. All of the data sets were then merged to produce the manageable set used here.

6. It seems unlikely that the results for the worker populations would be influenced to a large degree by an ecological aggregation effect, although such a possibility does exist with any analysis of aggregate data. The farm and urban results would appear less susceptible to such aggregation effects due to the relative homogeneity of these areas. However, the areas with large worker populations also tend to be heavily urban, and the results for the workers tend to have similar dynamic properties to that of the urban areas. Moreover, subsequent checking with alternative statistical strategies ranging from simple weighted correlations to more complicated logistic structures indicates that the present description of worker behavior is not a product of the particular model specification used here but rather a real structural characteristic of these data.

7. A conditional analysis similar to the above was also performed to test for differences in the partisan and new voter dynamics for the southern states as compared with the nonsouthern states. This analysis is not included in the body of the text only for reasons of space. However, the basic result is that there were no large differences in the directions of partisan and nonvoter change between southern and nonsouthern states (see also Shively 1971–72). This is not to say that the magnitudes of the static partisan totals for each election were similar, for obviously they were not. But the characteristics of change did not vary according to this division of region. In the southern states as well as the nonsouthern states, vote switching from the available pool of Republicans (admittedly small in the South) to the Democrats still dominated the 1932 election, whereas the new voters had their greatest impact in the 1936 contest.

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CLASS COMPROMISES IN INDUSTRIAL DEMOCRACIES

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The stability of democracies has been of critical interest to students of comparative politics. One question that has received limited empirical analysis is, How do we integrate class conflict into the broader concept of democratic stability? We extend the work of Przeworski and Wallerstein. Our principal thesis is that as the material bases of class exchange diminish, the state intervenes economically to expand the resources conducive to class consent. A tripartite coalition thus emerges, with the state facilitating class compromise between labor and capital through economic policies. We explore this thesis in a cross-national analysis of data collected from 20 advanced industrial democracies during the time period 1965–83, and explore the essential conclusions and the political-economic implications of our findings.

The stability of democracies has been of critical interest to students of comparative politics (Lijphart 1984; Powell 1982). One question that has received limited empirical analysis, particularly within a cross-national framework, is how class conflict can be managed so as to promote democratic stability.

Of course, this question presumes a special importance for class conflict in the organization of authority and the distribution of resources within contemporary democracies. Pluralist theory has essentially deemphasized class conflict, while the neocorporatist logic has broadened the concept through the introduction of social concertation, institutional cooperation, and interest coordination into the lexicon of class analysis (Korpi 1983). Others have argued that traditional class differences may be less critical to some democracies than religious, ethnic, regional, or racial concerns (Lijphart 1984), while a more recent body of literature has focused on the centrist tendencies in advanced industrial democracies (Jackman 1986). However, Hibbs has concluded that "trends in the strike activity tend to complement other streams of evidence contradicting macro-sociological arguments about the 'integration' of labor in advanced capitalist societies and the embourgeoisement of workers in the face of unprecedented affluence" (1976, 1057; see also Cameron 1984; Crouch 1985a; Korpi and Shalev 1979).

How, then, do we integrate class conflict into the broader concept of democratic performance? Przeworski (1980) and Przeworski and Wallerstein (1982) provide an answer. In contravention to the Marxian interpretation of class conflict as the product of two parties—labor and capital—unable to fashion agreement, Przeworski (1980) and Przeworski and Wallerstein (1982) formulate an alternative logic of class relations based on the capacity and willingness of labor and capital to consent to disagreement. Their research emphasizes the bases of class compromise and hence the preservation of democratic governance within capitalist economies. Class consent is made rational by the possibility of reproducing positive-sum—rather than zero- or negative-sum—relationships. Przeworski (1980) and Przeworski and Wallerstein (1982) draw attention to the economic, organizational, and political conditions conducive to reproducing class compromise.

What lies at the core of the process of renewing capitalism? Przeworski (1980) and Przeworski and Wallerstein (1982), as well as Lange, Ross, and Vannicelli (1982) argue that exchange is at the heart of stable (i.e., nonrevolutionary, nonviolent) class relations. Specifically, capital and labor exchange material benefits with the long-term objective of increasing the overall economic pie available for allocation. If this exchange ceases to be mutually beneficial, class compromise will falter, thereby threatening the replacement of democracy with capitalist dictatorship or labor autocracy. By illuminating the environmental and institutional factors critical to shaping class consensus, Przeworski (1980) and Przeworski and Wallerstein (1982) highlight the challenge public officials continually face, namely, as Powell reminds us, that of finding solutions to "major policy disagreements, as well as to recognized common problems, if the democratic system is to thrive" (1982, 227).

We attempt to extend the work of Przeworski (1980) and Przeworski and Wallerstein (1982) by examining the conditions that impel state interventionism in order to secure class stability. Our principal thesis is that as the material bases of capitalism diminish, the state intervenes economically in order to expand the resources conducive to class consent. A tripartite coalition thus emerges, with the state facilitating class compromise between labor and capital through economic policies. We explore the thesis in a cross-national analysis of data collected from 20 advanced industrial democracies during the 1965–83 time period.[1] For reasons discussed below, we use public employment as a measure of state intervention.

We first examine the logic behind the state's participation in economic affairs through the allocation of employment. We identify the aggregate resources available to capital for investment and the earning power of labor as proximal conditions motivating a tripartite (state, labor, and capital) coalition. Then we examine the dynamics behind changing the nature of such coalitions through expanded state involvement. The process of change is arguably different from the basic logic of state intervention in economic affairs. Finally, we examine a reduced-form model of the determinants of public employment and discuss its implications regarding class sovereignty. We also discuss the bases of manifest class consent, or—more accurately—the withdrawal of consent.

The Logic of Class Compromise and Tripartite Coalitions

In their recent extension of theories of class conflict, Przeworski and Wallerstein (1982) offer a powerful and elegant argument for conceptualizing class conflict in contemporary industrialized democracies. Challenging the neo-Marxist literature, their framework stresses the prominent role of an exchange system within class relations. The currency of exchange is material in nature. Capital agrees to withhold a portion of its savings for present wages and consumption, while workers agree to consent to capital's primary claim to profit.

While the consent of both labor and capital is necessary in order to achieve a system of exchange, Przeworski (1980,

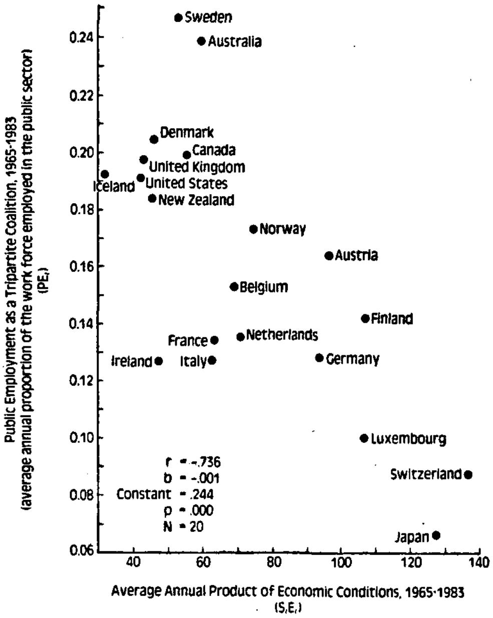
33) notes that such a system is nevertheless "hegemonic." The system depends upon the reproduction of profit. Thus the interests of capital within industrial democracies "represent the future universal interests while interests of all other groups appear as particularistic and hence inimical to future developments" (p. 27). Elaborating further, Korpi (1983, 16) distinguishes between the rather limited "power resource" available to labor (human capital) and the principal resource of capital (control over the means of production), which has "a large domain, wide scope and high concentration potential, as well as high scarcity and convertibility," all dimensions found lacking in human capital. Nonetheless, capital, in spite of its hegemonic advantage, must acquiesce in part to the interests of labor in order to prevent "those whose labor is extracted at any moment" from rebelling against the political-economic order (Przeworski 1980, 28). Further, labor organizes collectively in order to countervail capital's recognized power. Unions form for this purpose. This in turn provides the foundations upon which the "outcomes of conflict [between labor and capital] are within limits uncertain, and in particular, [foundations] in which these outcomes are not uniquely determined by class positions" (p. 28).

Przeworski and Wallerstein's (1982) theory identifies the role for the state in class compromise. In particular, the state is an instrument—a provider of economic resources to reproduce the material bases of capitalism in such a way as to assure labor's continual assent to the hegemony of profit. More specifically, they argue that "class compromise implies a particular organization of political relations, a particular relation between each class and the state, a particular set of institutions, and a particular set of policies" (Przeworski and Wallerstein 1982, 236). Therefore, state policies both manifest and support class compromise:

> These policies—and the state itself—now appear as an expression of a compromise: they are quite instrumental with regard to the interests of a class coalition that includes both capitalists and organized workers. When workers pursue strategies that lead to a compromise, the state does what appears necessary to reproduce capitalism because this is the choice of the workers as well as the capitalists. The organization of the state as an institution and the policies pursued by this institution constitute an expression of a specific class compromise. (p. 136)

A question thus becomes, What economic conditions lead to expanded state involvement (i.e., expanded public employment) and hence the material expression of a tripartite alliance? The answer lies in the logic which is behind any political coalition. For labor and capital, the logic of participating in a three-party coalition including the state is the imperative need to obtain additional resources that promote a capitalist system of exchange and thereby protect capital's sources of investment while also affording labor wages at sufficient levels to satisfy each. State involvement through public policies is a political-economic strategy to increase the material interests of both labor and capital. It is especially relevant to organized labor's continued assent to capitalism because it enables its leaders to assure members that public authority is being exerted to expand their purchasing power and, implicitly, to prevent the undue appropriation of profit by capital. As Edelman (1971, 144) observed, "Workers are likely to acquiesce without wide or deep dissent in the continued incumbency of their union officials as long as the economic benefits of an affluent society continue to roll in and as long as the union officials present an appropriate dramaturgical performance suggesting they are furthering the interests of their followers. In supplying both kinds of benefits, government can help union officials." Labor's continued confidence in the state as an instrument of economic expansion is

Figure 1. Public Employment as a Tripartite Coalition and the Product of Economic Conditions, 1965–83



arguably of more political importance than capital's support because of organized labor's electoral base (Edelman 1971; Przeworski and Wallerstein 1982). State involvement in class compromise through economic expansionism is therefore, as Przeworski and Wallerstein (1982, 236) point out, consistent with the role envisaged by Keynes (1936, 378): "It is not the ownership of the instruments of production which it is important for the state to assume. If the state is able to determine the aggregate amount of resources devoted to augmenting the instruments and basic rate of reward to those who own them, it will have accomplished all that is necessary [to assure class compromise]." In addition, the state itself, to the extent that it manifests capital's economic dominance, has a direct interest in intervening in behalf of class compromise.

The material bases of capitalism can be found, at least partly, in the product of two aggregated economic measures: S_tE_t. In our cross-national analysis, S_t is the average ratio of gross savings to the gross domestic product in an industrial democracy for the 1965–83 time period, and E_t is the average annual ratio of the rate of growth in real weekly earnings in manufacturing to the rate of growth in the consumer price index in an industrial democracy for the same period.[2] S_t indicates the resources available to capital for reinvestment, and E_t suggests how labor is benefiting within economic systems in terms of growth in their real capacity to consume goods and services.[3] Our expectation is that the larger the product of S_tE_t, the more likely labor and capital will be able to fashion a positive-sum exchange and class compromise. The incentives for the state to expand its economic interventionist policies are accordingly less. Conversely, a smaller product implies weakened material bases of class consent and hence greater need for state involvement. For instance, declining growth or negative growth in real earnings may cause workers to have less confidence in their future earning power, thereby generating political pressure for governmental intervention. We therefore expect the product S_tE_t to be negatively correlated with public employment in this model: $PE_t = f(S_tE_t)$, where PE_t is the average annual proportion of the workforce of an industrial democracy employed in the public sector for the years 1965–83.[4]

Figure 1 plots the relationship between PE_t and S_tE_t across 20 democracies.[5] The data generally support the expected negative relationship. Public employment is generally higher in countries with lower material bases, as expressed in the S_tE_t product.

At least two other factors, however, are likely to effect the participation of labor, capital, and the state in tripartite coalitions to support class compromise through public employment. One is the extent to which class relationships are formally organized or institutionalized. Crouch (1985b), Lange (1984), Lange and Garrett (1985), and Przeworski and Wallerstein (1982) provide a theoretical basis for this expectation. The presence of institutionalized procedures for resolving economic differences (over how to slice the economic pie) implies a spirit of compromise between labor and capital—or at least a willingness to mitigate the potential for class warfare. This problem-solving spirit might spill over into political relations (Goldthorpe 1984, 328). To the extent that capital and labor benefit from these institutional arrangements, they have an incentive to promote state policies that reinforce the bases of compromise.

The stability of institutionalized relations (manifested, for example, in the form of collective bargaining) depends substantially on the institutional position of organized labor in society. Lange (1984) and Lange and Garrett (1985) have argued that strong, centralized labor organizations provide an incentive for capital to work toward a compromise with labor and labor to reduce its militancy relative to capital. Such institutional arrangements enhance capital's confidence that organized labor will guarantee the compliance of its member organizations in accordance with the terms of any wage and benefit contracts agreed to by organized labor. Furthermore, centralized labor organizations enhance wage earners' confidence that organized labor will be able to extract the necessary concessions from capital in industrial negotiations. Przeworski and Wallerstein have suggested that in a more general sense the capacity of labor and capital to agree upon a system of exchange in order to achieve class consent will be enhanced when capital is certain that "particular groups among [labor] would not conclude their own agreements with their respective employers at the cost of other workers" (1982, 220). One may infer from the potential significance of these institutional arrangements that both classes have a vested interest in supporting state policies that reinforce confidence in the extant consent and that do not sow discord between and within labor and capital.

Although a variety of operationalizations of institutionalized class relations exists, Cameron (1984) provides the most sophisticated index. The measure essentially captures the centralization of organized labor and its integration within the capitalist structure. The components of it are (1) the level at which labor bargains, (2) the unity of labor's organization, (3) the style of political-economic decision making within which labor and capital coordinate their relationship, (4) the strength of labor unions within the work place itself, and (5) the density of labor union organizations within the national economy. The first four variables comprise a separate subindex of organizational interaction for each country in our sample. The value of this separate subindex was then combined with the union density value for each country, and from this an index of institutionalized class relations (ICR_t) was devised.[6]

A second factor may also affect the incentives of the classes to compromise and the basic capacity of a state to invest in public employment. Broadly speaking, Przeworski and Wallerstein (1982) have alerted us to the role that international economic developments might play in shaping the parties' confidence in the positive-sum aspects of class compromise. Such compromise depends at least partly on the "ordinary risks . . . owing to domestic and international fluctuations, domestic and international competition" (Przeworski and Wallerstein 1982, 221).

More specifically, Alt (1985) has identified a society's openness (i.e., the extent to which a country relies upon foreign trade, particularly exports) as an important factor in determining a state's capac-

Table 1. Determinants of Variation in State Intervention via Public Employment, 1965–83

Independent Variables	Unstandardized Regression Coefficient (b) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)
Economic conditions (S_tE_t)	–.001**	–.745**
	(.000)	(–5.828)
Institutionalized class relations (ICR_t)	.083*	.450*
	(.024)	(3.397)
Exports/GDP (EXP_t)	–.078*	–.279*
	(.037)	(–2.100)

Note: Dependent variable = average annual proportion of the work force employed in the public sector. Adjusted $R^2 = .699$ ($F = 15.694$)**. Constant = .228 ($T = 11.646$)**. Number of cases = 20.

*$p \leq .05 > .01$.

**$p \leqslant .01$.

ity to influence employment through public policies.[7] Openness might also affect the incentives on the part of labor and capital to compromise in the first place and to support state interventionism. It is not possible, however, to predict a priori the direction of this effect. It depends on the conditions of international economies in general and trading partners in particular. For instance, "If the economies of a country's major trading partners slow down, demand for that country's exports will decline" (Alt 1985, 1020), thereby raising unemployment and reducing public "resources."[8] At the same time, it automatically triggers some spending in the form of unemployment assistance, and this naturally further shrinks the monies available for other purposes. To compensate for these effects, government must either increase taxes (by expanding existing rates or taxing new sources) or incur debt. At the same time, however, this situation might generate additional political pressures to stimulate economic activity through expanded public employment and the like. In short, openness acts as a conditional factor, affecting—albeit ambiguously—the incentives to expand or stabilize public employment.

Table 1 presents the results of an empirical analysis of the impact of ICR_t and exports (EXP_t)—in addition to S_tE_t—on public employment. The results support the S_tE_t and ICR_t hypotheses. The S_tE_t result indicates that the aggregate economic bases of class compromise are negatively associated with public employment levels across industrial democracies. ICR_t creates conditions favorable to higher levels of such employment, whereas the EXP_t result suggests that openness tends to restrict public employment. Altogether, the multivariate analysis gives us more confidence in the plausibility of the $S_tE_t - PE_t$ relationship.

Changing Nature of Tripartite Coalitions

The logic of a tripartite coalition reflects the basic economic need for the state to reinforce the bases of class consent through public policies. Intercountry differences in the size of public employment, however, depend not just on the material bases of capitalism (profit and purchasing power) but also economic openness and the institutionalization of class relations. It is arguable, however, that the dynamics involved in changing public employment at any given point in time may somewhat differ.

The capacity and willingness of capital, labor, and the state to change public employment (i.e., to adjust the rate of growth in such employment) rests on at least two related conditions. The first is the political complexion of a society (Alt 1985; Cameron 1982; Lange and Garrett 1985; Przeworski and Wallerstein 1982). In this regard, the ideological center of gravity becomes relevant. Generally speaking, labor's political influence is somewhat tied to the power of leftist parties. Leftist governments presumably respond more favorably to organized labor's public policy preferences, especially to the extent that it is a key part of a winning electoral coalition. Capital is in a weakened position to offer resistance to labor and its leftist allies lest it might thereby engender more militant actions. Therefore, to the extent that organized labor generally seeks an expanded public sector, greater rates of growth in public employment should be exhibited in nations where the ideological center is on the left of the continuum.

The parties' confidence that an increase in the rate of growth in public employment will translate into greater benefits in the future is the other consideration. The predictability and stability in class relations will obviously influence their confidence. Unpredictable and unstable relations make capital and labor each uncertain as to whether the other's industrial behavior will be adjusted in order to promote mutually beneficial exchanges within an expanding public sector economy. Both parties are arguably interested in containing excesses in such behavior in order to reproduce the material bases of capitalism. Here the institutionalization of class relations assumes much practical import. As previously noted, such institutionalization formalizes and regularizes relations. An encompassing, centralized labor movement may be better able to contain short-term benefits (Maitland 1985; Reshef 1986). With this condition in place, the resistance to a change in public policy may wane, ceteris paribus. Thus if, as before, we define the institutionalization of class relations as ICR_t and if we define labor's political success as the extent to which the center of ideological gravity is leftist, we may test the following model: $\Delta PE_t = ICR_t + \bar{I}_t$, where ΔPE_t is the average annual rate of growth in the proportion of the total work force employed in the public sector for a country between the years 1965 and 1983, and I_t is the average annual ideological center of gravity within the lower chamber of the national legislature between years 1965 and 1983.[9]

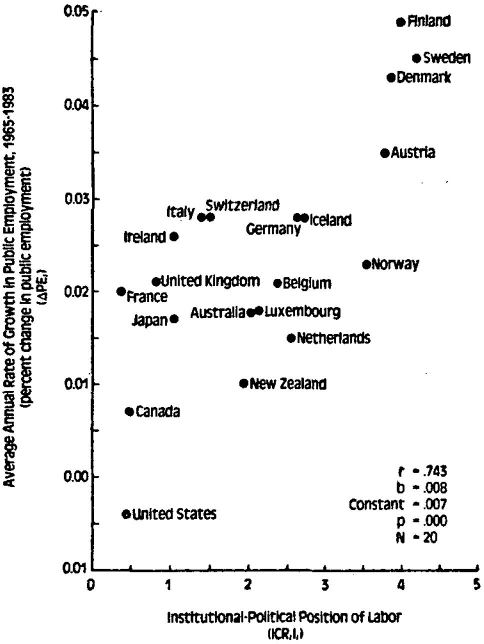
Based on the data, we derive the following estimates:

$$\Delta PE_t = .022 + \underset{(.010)}{.024(ICR_t)} + \underset{(.005)}{.008(I_t)} + e, \qquad (1)$$

where the standard errors are indicated in the parentheses below each unstandardized regression coefficient. While the model can account for nearly half the variance ($R = .701$) in ΔPE_t, the presence of institutionalized class relations (ICR_t) is slightly more influential than the ideological center of gravity (I_t) within a country. Equation 1 suggests that the increase in I_t, denoting a more pronounced leftist orientation within the legislature, does tend to increase the expansion of tripartite coalitions, yet not as significantly as the institutionalization of class relations (ICR_t).

Rather than conceptualizing institutional class relations and labor political power as two separate and independent influences on the state, it would seem that they are interdependent. To some extent, institutional and political power go hand

Figure 2. The Rate of Growth in Public Employment and the Institutional-Political Position of Labor, 1965–83



in hand (Reshef 1986; Lange and Garrett 1985), mutually reinforcing each other. As Lange and Garrett (1985) argue, "The organizational and political power of labor both strongly affect economic growth. The impact of each, however, is heavily contingent upon the relative presence or absence of the other" (p. 820). A strong labor political presence, coupled with a well-established institutional framework for handling industrial disputes and differences, may multiplicatively affect a state's policy determinations, especially to the extent that such determinations must "win popular support in elections" (Przeworski and Wallerstein 1982, 236). In a similar vein, such a combination should lessen capital's incentive to resist expanded public sector involvement by making the expected costs of resistance more predictable (i.e., increased labor militancy in economic and political affairs).

This interactive proposition may be formulated as

$$\Delta PE_t = f(ICR_tI_t), \qquad (2)$$

where ICR_tI_t reflects the combined impact of labor's institutional and political position within a country. Figure 2 plots this relationship across the 20 industrialized democracies. The model improves the total explained variance of ΔPE_t, from 49.1 in equation 1 to 55.2 in equation 2. Where class relations and political representation are reinforcing, offering capital and labor greater certainty as to their position within a country's political economy, barriers to expanding the public sector become significantly more manageable and surmountable than in those democracies where the political and institutional class representation have been less mutually reinforcing.

An examination of Figure 2 suggests the possibility, however, that the relationship between ICR_t and ΔPE_t may be the result of the means by which interests in general are channeled into the political arena. Employing Lijphart's models of democracy (1984, 21–45), we can see that 5 of the 10 countries (Australia, Canada, New Zealand, the United Kingdom, and the United States) that fall at or below the median point (.021) for ΔPE_t exhibit predominantly majoritarian characteristics with respect to the style of executive-legislative relations and issue representation. Based upon an exclusionary principle common, although not exclusive, to Anglo-American democracies, the majoritarian style of representation tends to reward those interests within the mathematical majority, while largely excluding those interests that fall outside the range. Above the median point of ΔPE_t, we find 3 of the 10 countries (Denmark, Finland, and Switzerland) to be those that exhibit the consensual style of executive-legislative relations and issue representation. Consensual executive-legislative relations are more likely to preserve the specificity of competing interests while seeking accommodation through "grand coalitions" (Lijphart 1984). Majoritarian executive-legislative relations, by contrast, would tend to force interests into an aggregated bundle, thereby inhibiting reform and change. The implication follows that the expansion of political-economic coalitions may have less to do with ICR_tI_t per se, than with institutional arrangements that have been sustained to allow a variety of interests to find assistance and attention through public policies. Issues are defined, presented, and eventually implemented through three critical institutions in democratic societies: parties, legislatures, and executives. Party polarization and sharp issue cleavage tend to intensify political conflict and more clearly distinguish political interests within democracies (Lijphart 1984; Powell 1982). This condition is further aggravated when the principal chamber of the legislature is fractionalized among several different political parties, thus tending to disaggregate and extend the number of distinct interests represented within the legislative arena of modern democracies (Rae 1971). Finally, if either of these two conditions are complemented by an institutionalized executive formula that departs from a simple majoritarian model, wherein the executive is not concentrated in one person or political party controling a majority of seats in the principal chamber of the legislature or where the executive does not consist of a minimum winning coalition, the tendency toward consensus and disaggregation is increased (Dodd 1976).

While it is customary to examine one—or possibly two—of these three factors when operationalizing the degree of issue disaggregation within democracies, we have opted for a weighted index that concentrates explicitly upon those three factors critical to the style of executive-legislative decision making. Therefore, for each democracy within our sample we have separately determined for the years 1965–83 (1) the degree of dimensional polarization within the party system,[10] (2) the degree of fractionalization within the lower chamber of the national legislature,[11] and (3) the extent to which the power of the executive is diminished relative to the legislature.[12] Having obtained these individual measures for the three separate indicators of issue disaggregation within the 20 democracies, we then derived separate weighted index scores for each democracy by factor-analyzing the data and obtaining standardized factor scores from this procedure. The larger the value of the factor score, the more the style of executive-legislative relations assume the characteristics of the consensual model of democratic representation and departs from a style characteristic of the majoritarian model of democratic representation.[13]

A second characteristic of these democracies that may have an independent impact upon the incentives to change public employment is the degree of political-economic instability within the industrialized democracy. Instability within the political environment is tantamount to an increase in the insecurities of both labor and capital regarding their future material interests as served by the present configuration of tripartite coalitions. As Lange notes, "Whatever the political composition of the government, if the state is not perceived to be able to implement its policy intentions, workers' insecurities about the future prospects are likely to be increased" (1984, 111). We presume that the same concerns influence capital. Thus partisan alignments upon which stable policies are built should be a critical factor in defining these coalitions. We would expect that the more unstable such partisan alignments or the more volatile the political environment, the more resistant

Table 2. Rate of Growth in Public Employment, 1965–83

Independent Variables	Unstandardized Regression Coefficient (b) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)
Institutional-political position of labor (ICR_tI_t)	.006** (.001)	.633** (4.232)
Style of executive-legislative relation	.005* (.002)	.374* (2.400)
Political-economic instability ($RV_t\Delta UR_t$)	-.204* (.101)	-2.91* (-2.101)

Note: Dependent variable = average annual rate of change in the proportion of the work force employed in the public sector (ΔPE_t). Adjusted $R^2 = .641$ ($F = 12.284$)**. Intercept = .013 ($T = 3.223$)**. Number of cases = 20.

$^*p \leqslant .05 \geqslant .01$.
$^{**}p \leqslant .01$.

the parties will be to the transformation and modification of state intervention in economies through expanded public employment.

The effect of political volatility on labor and capital's ability to reach accord on expanding coalitions should, however, be conditioned by their respective evaluations of the economic environment and how it will affect the future material interests. Again, Lange (1984) underscores this point when he suggests that labor and, by inference, capital "can be expected to evaluate the political 'guarantees' about future benefits from [class coalitions] in the context of their perceptions of the more general prospects for future economic performance as determined by domestic and international conditions [and] how government can help reduce workers' [and capital's] uncertainties about the future" (p. 111). Therefore, political volatility and economic instability should have a decisive and interdependent influence on the nature of tripartite coalitions. The greater the presence of political-economic instability, the less we should expect capital and labor to reach the kind of consensus necessary to alter public policy regarding the role of public employment. To test this hypothesis, we have defined political-economic instability as $RV_t \Delta U_t$. RV_t is the average annual volatility of representation within a democracy between 1965 and 1983, and ΔU_t is the average annual rate of growth in the proportion of the work force unemployed for the years 1965–83.[14]

Table 2 presents the estimations of the rate of growth in public employment across industrial democracies. The results of this analysis confirm the proposition that ICR_tI_t is the proximal source of the rate at which coalitions expand (b = .006). Nonetheless, the style of executive-legislative relations and political-economic instability are both significant —though secondary—factors in determining this rate of expansion (b = .005 and b = .204, respectively). Both coefficients are in the predicted directions. We may conclude therefore that the rate at which tripartite coalitions expand through the enlargement of public employment is dependent primarily upon the institutional and political position of labor and the assurances of restraint and cooperation that these mutually reinforcing conditions imply. This relationship holds in spite of nature of executive-legislative relations within industrialized democracies—and to the degree to which the political-economic environment of the democracy is both unstable and uncertain.

The Logic and Nature in Perspective: Class Sovereignty and Conflict

Class Sovereignty

Up to this point, our analysis has underscored the conclusions of Przeworski (1980) and Przeworski and Wallerstein (1982): there is a strong material basis (S_tE_t) to class compromise, a process additionally conditioned by the political, institutional environment, especially the institutionalization of class relations. There is, however, a fundamental consequence that lies in the core of tripartite coalitions: the decline of class sovereignty. If we define class sovereignty as the extent to which labor and capital restrict the class coalition to only two parties—labor and capital—we may understand this sovereignty to represent the capacity and willingness of capital and labor to structure their exchange system and organize their relationship according to a more exclusive set of preferences, primarily those of their memberships and organizational leadership.

With public authority comes an expansion of the issues that consume public leadership. Schattschneider (1975) identified this process as the contagion of conflict, which spreads as greater numbers of interests find their expression in the arena of political struggle. Private conflicts become public conflicts, and policy debate becomes socialized through the inclusion of public authority. Schattschneider (1975), though speaking with reference to the U.S. system, underscored the resulting decline in class sovereignty when he observed, "It is the function of public authority to modify private power relations by enlarging the scope of conflict. Nothing could be more mistaken than to suppose that public authority merely registers the dominance of the strong over the weak" (p. 40). Across democracies in general, the growth of public authority expands rule-making and bureaucratic procedure, which further diminishes the ability of private groups to define the issues of political contest (Aberbach, Putnam, and Rockman 1985; Dogan 1975; Page 1985b; and Suleiman 1974).

The logic and nature of class compromise, reinforced by tripartite coalitions involving state intervention, reveal the principal factors that determine the scope (or relative size) of class sovereignty in advanced industrial democracies. This scope will be no larger than the evident need to revitalize and supplement the exchange system of capital and labor through the addition of public resources as measured by PE_t. This need is reflected in investment and earnings resources (S_tE_t). However, this scope will be no smaller than the combined institutional and political power of labor (ICR_tI_t) allow. Furthermore, given a predetermined or extant need for government involvement, the flexibility to change the rate of involvement will similarly depend on ICR_tI_t.

These propositions are explored in Table 3. While ICR_tI_t clearly both constricts the scope of class sovereignty by increasing the role of public authority—as measured by PE_t—in class relations (b = .014) and reduces the uncertainties that can encumber change in public employment (b = .007), S_tE_t is the principal determinant of the actual configuration of tripartite coalitions (b = −.001) and therefore the scope of class sovereignty. The findings reported in Table 3 confirm the central dynamic and the predominate logic of class compromise and tripartite coalitions: regardless of the degree to which capital and labor have over time forged a sound and adherent relationship within the political and economic arena of

Table 3. Estimates of PE_t and (ΔPE_t), Employing Only Proximal Determinants of the Logic and Nature of Tripartite Coalitions, 1965–83

	Model 1: Estimating (PE_t)		Model 2: Estimating (ΔPE_t)	
Independent Variables	Unstandardized Regression Coefficient (b) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)	Unstandardized Regression Coefficient (b) (Standard Error)	Standardized Regression Coefficient (β) (T-Statistic)
Economic conditions (S_tE_t)	–.001** (.000)	–.760** (-5.118)	.000 (.000)	.154 (.952)
Institutional-political position of labor (ICR_tI_t)	.014* (.005)	.379* (2.554)	.007** (.001)	.733** (4.539)
Adjusted R²	.609 (F = 15.007)**		.537 (F-statistic = 11.433)**	
Constant	.217 (T-statistic = 10.716)**		.003 (T-statistic = .529)	
Number of cases	20		20	

*$p \leqslant .05 > .01$.
**$p \leqslant .01$.

capitalist democracies, the final configuration of government's role in class compromise rests ultimately on how the material interests of labor and capital cumulatively affect the relations on which the system of class exchange is built.

Class Conflict

We pursue the implications of our analyses by examining the relationship between the salient determinants of public employment—regarded as a tool of a tripartite coalition to reinforce class compromise—and the presence of class conflict in industrial democracies. We have argued, with respect to public employment levels per se, that a deterioration in the material bases of capitalism (S_tE_t) should contribute to greater state interventionism. Such a deterioration does indeed seem to be associated with a presumably corrective public policy response, but at the same time it might strain class relations by lessening confidence in the positive-sum aspects of class compromise. Therefore, weakened economic conditions, as reflected in lower levels of S_tE_t, might contribute to outbreaks of class conflict.

Based on our analysis of both public employment levels and change in public employment growth rates, however, such conflict should be modified by the combined institutional and political position of labor (ICR_tI_t). The institutionalization of class relations gives labor the right and power to obtain benefits without having to engage rampantly in open conflict with capital. Favorable political representation gives unions greater confidence that the state will intervene in order to expand the material bases of class compromise and thereby increase the real earning power of workers.

Although public employment may be a direct manifestation of class compromise and a tripartite coalition, its impact on class conflict, especially in the short run, is problematical. In this vein, state interventionism through public employment represents a response to underlying economic variables, conditioned by institutional political considerations. Public employment is therefore arguably endogenous in the short run, and its expan-

Table 4. Regression of Estimates of the Withdrawal of Class Consent, 1965–83

	Model 1: Estimating (WC_t)		Model 2: Estimating (WC_t)		Model 3: Estimating (WC_t)	
Independent Variables	*b* (Standard Error)	β (T-Statistic)	*b* (Standard Error)	β (T-Statistic)	*b* (Standard Error)	β (T-Statistic)
Economic conditions (S_tE_t)	−.005* (.002)	−.573* (-2.021)	−.005 (.001)	−.534** (5.212)	—	—
Institutional-political position of labor (ICR_tI_t)	−.122* (.061)	−.581* (-2.000)	−.088* (.034)	−.443* (-2.586)	—	—
Public employment (PE_t)	−.070 (1.679)	−.012 (−.039)	—	—	1.328 (1.211)	.244 (1.096)
Growth in public employment (ΔPE_t)	3.813 (5.555)	.190 (.600)	—	—	-7.563 (4.360)	−.380 (-1.731)
Adjusted R² (F-statistic)	.426 (4.341)**		.480 (9.299)**		.105 (2.057)	
Constant (T-statistic)	1.173 (2.995)**		1.170 (9.223)**		.624 (2.743)**	
Number of cases	19		19		19	

*$p \leqslant .05 > .01$.
**$p \leqslant .01$.

sion as a matter of policy may coincide with conflict to the extent that the seeds of conflict also spawn corrective or ameliorating public policies.

If we define open industrial conflict as a proxy measure of the extent to which labor and capital withdraw their consent, we can test the following models:

$$WC_t = f(S_tE_t) + (ICR_tI_t) + (\Delta PE_t) + (PE_t)$$

$$WC_t = f(S_tE_t) + (ICR_tI_T)$$

$$WC_t = f(PE_t) + (\Delta PE_t).$$

Operationalizing the withdrawal of class consent (WC_t) as the average annual volume of strike activity (logged) in a democracy for the period 1965–83, Table 4 confirms our expectations.[15] Three conclusions emerge from the empirical analyses. First, the principal contributor to conflict is the product S_tE_t, which suggests, as predicted, that a deterioration in the material bases of capitalism engenders class conflict (but also, as the logic of class compromise indicates, the public policies suited for amelioration). Second, the combined institutional and political position of labor, as reflected in well-established class relations and favorable political representation, apparently reduces conflict, suggesting that labor is confident of benefiting enough in the long run from compromise and consent to limit short-term hostilities. Third, public employment, as well as the rate of growth in public employment, does not significantly affect industrial conflict across industrial democracies.

Conclusions and Implications

We have attempted to extend the works of Przeworski (1980) and Przeworski and Wallerstein (1982) in particular. These scholars have laid the theoretical basis for class compromise, arguing that it is in the material interests of capital and labor to reinforce the bases of their exchanges. Our principal thesis is that there is an economic logic, based on the material conditions of exchange, that provides the incentive for the state to intervene in economic affairs in order to stimulate the bases of capitalism and class compromise. Through its intervention, the state essentially enters into a tripartite coalition with capital and labor, a coalition logically serving the interests of the two classes, at least in the longer term. Focusing on public employment as a measure of state intervention, our empirical analyses across 20 advanced industrial democracies support the logic. They also show that the institutional and political position of organized labor affects the capacity and willingness of the state to expand the tripartite coalition: greater rates of growth in public employment are associated with greater levels of institutionalized class relations and prolabor political representation.[16]

Three main conclusions may be derived from our analyses. First, economics lies at the heart of class compromise and class conflict, and at establishing the means of resolving conflict through expanded state interventionism. Tripartite coalitions are economically induced phenomena aimed at reproducing the positive-sum aspects of economic exchanges between two competing classes—labor and capital.

Second, politics are relevant to the degree to which the parties will assent to an expanded level of state involvement. It registers both the degree of confidence the parties, especially labor, have in the likelihood that their interests will be fairly and adequately protected. It also manifests the degree to which one party can resist the public policy preferences of another. Where labor dominates politically, capital is obviously less able to forestall its preferences. Indeed, recalcitrance might breed worker militancy, thereby threatening the very foundation of capital. Ironically, organized labor's political power may be positively associated with capital-labor cooperation in politics. In a related vein, institutional and political position, when positively correlated, interacts synergistically.

Third, the institutionalization of class relations, part of which involves the degree of union penetration in the labor force, facilitates state intervention through public employment and reduces manifest class conflict. Formal procedures through which labor is given the opportunity and right to represent its economic interests secures rather than destabilizes capitalism. In other words, rather than undermining capitalistic democracies, a free and powerful labor movement may well add a degree of certitude and confidence essential to stability in democratic performance.

Two principal implications follow. The first is that the state should evaluate the merits of its macroeconomic policies at least partly in terms of the impact they will have on economic conditions that affect the willingness of capital and labor to compromise. Governments should look on these policies as means of influencing confidence in the parties' belief that they will benefit in the long term from consensual behavior. This implies that unstable, erratic policies may erode such confidence and thereby precipitate conflict.

The second is that the logic of class compromise and consent developed by Przeworski (1980) and Przeworski and Wallerstein (1982) is a powerful theoretical argument. The inherency of class conflict built into Marxist thought is certainly called into question: economic and political conditions may either provoke or stymie conflict. This is not to suggest, however, that class differences are perpetually evanescent. The essence of compromise is the exchange of resources and authority required to contain different interests. Nonetheless, compromise and its reproduction are clearly attainable.

Notes

1. The countries in our analysis are Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, Japan,

Luxembourg, the Netherlands, New Zealand, Norway, Sweden, Switzerland, the United Kingdom, and the United States.

2. Data for S_tE_t were collected from International Labour Office 1965-85 (various volumes) and Organization for Economic Cooperation and Development (hereafter OECD) 1982b, 1984b, and 1986 (p. 158). The mean value of S_tE_t across the sample was 71.66 (standard deviation = 29.922), with a minimum value of 32.12 (Iceland) and a maximum value of 136.75 (Switzerland).

3. We focus on S_t and E_t in particular, rather than an aggregate measure of economic performance (e.g., gross domestic product growth rates), because we are interested in tapping the resources available to competing classes. Savings are essential to the capacity of capital to reinvest. Factors that threaten savings undermine a society's ability to reproduce capitalism (Musgrave and Musgrave 1976; Przeworski and Wallerstein 1982). We believe that growth in real earnings is especially critical to organized labor's political-economic calculations regarding the merits of consensual behavior. In the present, workers sacrifice income, and hence consumption, in the hope of expanding the future base of their rewards by allowing capital greater capacity to expand. To some extent, however, they must rationally discount future benefits, and it is arguable that their discount *rates rise in relation to the confidence they have that* future rewards are forthcoming (Przeworski and Wallerstein 1982). Rising real income would seem to root such psychological confidence in tangible economic performance (McCallum 1986).

4. We need to explain why we chose public employment as our dependent indicator of state interventionism over more common measures based on public finance (see, for instance, Cameron 1978). First, as Rose (1985) and many others have documented, public employment has grown significantly in most industrial democracies since World War II. Many factors explain this growth, but governments can and do expand public employment for the sole purpose of stimulating economic growth (Musgrave 1982). Capital and labor will often combine to push for such an expansion, as indicated by the agreement reached between capital and labor in the Spanish shoe industry in 1980 to seek state intervention to protect employment in that industry (International Labour Office 1980, 441). (We should note that we reviewed the *Social and Labour Bulletin* for the 1965-83 period, and identified numerous examples of the state intervention in employment.)

Furthermore, governmental programs aimed at shielding workers and their families from the harshness of industrial life—programs in such areas as education assistance, health care, and job training—entail employment. As these programs expand, so does governmental employment.

Second, public employment "is not as fungible as money." It is a more permanent form of public commitment and, by implication, a more significant intervention in economic affairs. Third, public employment is easily conceived of in terms of expenditures. For many governments, personnel is the single largest cost. Public employees are paid salaries and benefits—public expenditures.

The measure of public employment is the total work force employed in the various departments and agencies at central, state, provincial and local levels of government that "produce non-market goods and services" (OECD 1982a, 9). While the collection of public employment data is known to vary somewhat across countries (Economic Commission for Europe 1979; Haveman and Halberstadt 1982; Heller and Tait 1984; OECD 1982b; Page 1985a; and Rose 1985), the data provided by OECD (1982b), supplemented by equivalent data from OECD (1984a, 1985), Rose (1985), and Martin (1982) provide a reasonable cross-nationally equivalent measure of public employment at the level of general government. The mean value of PE_t in the sample is .159—15.9% (standard deviation = .048) with a minimum value of .066 (Japan) and a maximum value of .246 (Sweden).

In subsequent analyses, we focus on the average annual rate of change in public employment (ΔPE_t) between 1965 and 1983, arguing that the dynamics of change may be somewhat different from the *explanation of rates of public employment across* nations. The average annual rate of change in public employment across nations between 1965 and 1983 is .024 (standard deviation = .013) with a minimum value of −.004 (United States) and a maximum value of .049 (Finland). Our theoretical model and empirical analyses are intended to explain intercountry variation in PE_t and ΔPE_t.

5. The unit of analysis in this study is a country-time period (1965-83). Choosing a longitudinal time period was made necessary partly because several variables in the subsequent portion of our study are not available across our sample on a continuous annual basis (e.g., public employment and strikes); others do not vary on an annual basis (e.g., elections, trade union density, institutionalized class relations, ideological center of gravity within legislatures, and executive-legislative relations). Although a continuous time series analysis is impossible, we can capture the relationships between variables averaged over several years, thus avoiding the problem introduced by yearly aberrations within year-specific cross-sectional analyses.

6. Bargaining level is assigned a value of one in countries where bargaining occurs at a regional or national level, and zero is assigned to those countries where bargaining is decentralized at the enterprise or work place level (Blanpain 1985; Blythe 1979). The unity measure assigns one to countries in which a confederation dominates the labour movement and zero to countries in which there is no singular dominant confederation (Blanpain 1985; Blythe 1979).

Countries that exhibit a style of political-economic decision making that actively and systematically integrates labor, capital, and government (neocorporatist) are assigned a value of one; and those not exhibiting such a style of political-economic decision making are assigned a value of zero (Crouch 1985a, 117). Works councils are assigned a value of one in countries where workers have the statutory right to participate in works councils, and zero is assigned to all other countries (Cameron 1984, 164–65). The value of each of these four indicators was constant for a country over the entire time period 1965–83, as annual estimates are both highly inaccurate or incomplete across the sample. Whereas Cameron (1984, 164–65) has measured these variables at the interval level, we have opted for a dichotomous measure because of the ambiguity associated with some of the measures for countries not included in Cameron's sample. We have summed these four dummy variables for each country and divided by four to derive the subindex of organizational interaction for each country. This value is then added to union density (.001), and this sum halved to calculate the final measure of institutionalized class relations (ICR_t). For a similar procedure, see Lange and Garrett 1985. Union density is a measure of the proportion of the work force in labor unions. These are estimates that vary in accuracy and frequency across the 20 democracies (Blanpain 1985; Crouch 1985a, 114–15). The value of union density for each country during the period 1965–83 is based upon the average annual estimates available for that country. Data on union density are taken from numerous sources, the principal ones being Troy and Sheflin (1985), Blanpain (1985), Coldrick and Jones (1979), and Bain and Price (1980). The mean value for institutionalized class relations for the sample is .498 (standard deviation = .259), with a minimum value of .107 (France) and a maximum value of .917 (Sweden).

7. Data on exports were taken from OECD 1986. The mean value in our sample is .317 (standard deviation = .171) with a minimum value of .072 (United States) and a maximum value of .828 (Luxembourg).

8. Unemployment reduces the income base from which states may generate revenues for public employment.

9. We have followed the sample of Gross and Sigelman (1984, 466–67) and employed the following formula to obtain the value of a country's ideological center of gravity:

$$\sum_{i=1}^{n} (TiCi)/19$$

where T is the proportion of seats of a particular political party in the lower chamber of the legislature, C is the party's position along a 10-point ideological left-right continuum, and i is the particular legislature between 1965 and 1983. Between general elections for the legislature within a particular country, the value of (I_t) for the last general election was assigned for each year. During those years when general elections to the legislature occurred, the value of that year's ideological center of gravity was assigned its value based on that year's general election if the election occurred before 30 June; the value of the year prior to the general election was assigned if the election occurred after 1 July of the year. We have employed Gross and Sigelman's (1984, 469) example and relied on the 10-point left-right ideological scale as provided by the *Britannica Book of the Year* (for the years 1970–83). The scale ranges from 0 (fascist) to 9 (communist), with 4.5 as the center. Where codes changed over the time period—as a few did—these were incorporated into our calculation of (I_t). In the case of the United States, the House of Representatives was used as the proxy "principal" chamber. Data on proportions of seats in the legislative were taken from Mackie and Rose (1982) and *Keesing's Contemporary Archives* 1965–84 (for the later years). The mean value of (I_t) for the sample is 4.218 (standard deviation = .514) with a minimum value of 3.440 (United Kingdom) and a maximum value of 5.274 (Italy).

10. Dimensional polarization is an index designed to measure the extent of party ideological polarization within the context of the number of issues which divide the party between 1965 and 1983. The calculation of dimensional polarization is based on the product P_tD_t, where P_t is the extent of party ideological polarization (along a standard left-right spectrum) between 1965 and 1983, and D_t is the number of issue dimensions that have divided the country between 1965 and 1983. Following the example of Dodd (1976, 105–6), P_t is calculated according to the formula

$$\sum_{i=1}^{n} \Big(\sum_{ci=1}^{n} V_{ci} \Big)/19,$$

where V_{ci} is the variance of party i's position on a standard 10-point left-right scale in the legislature. V_{ci} is defined further as

$$\Sigma\,(f_i\%)\,(X_{ij} - \bar{X}^2),$$

where X_{ij} is party i's position on the Britannica's 10-point left-right scale and X is the mean of all X_{ij}s, each weighted by the percentage of legislature seats associated with party i in the legislature ($f_i\%$). As with I_t, for each year between general elections, the value of P for the last election is assigned for a given year. During general election years, the value of P is decided according to when the election is held. The number of issue dimensions for a country P_t is taken from Lijphart 1984 (p. 130). The difference between P_t and $P_t - D_t$ can be dramatic. In the case of Austria, for instance, $P_t = 1.181$; and in Sweden $P_t = 1.846$. In Austria, there are 2 issue dimensions, in

Sweden 2.5. The respective index values (P_tD_t) are 2.362 and 4.615, once P_t is weighted by the issue environment within which the left-right polarization exists. Thus the differences in terms of each county's ability to resolve political conflict through a majoritarian form are more sharply highlighted when examining dimensional polarization (P_tD_t). The mean value (P_tD_t) across the sample is 4.286 (standard deviation = 2.597), with a maximum value of 8.323 (Finland) and a minimum value of .344 (Ireland).

11. Fractionalization is defined according to the standard formula

$$1 \sum_{i=1}^{n} (T_i)^2/19$$

where T_i is party i's proportion (.001) of seats in the lower chamber of the legislature (Rae 1971). The larger the value of fractionalization, the greater the probability that any two seats in the legislature will be held by different political parties. The mean value across the sample is .673 (standard deviation = .103). The high value was Finland (.814), and the low value New Zealand (.489).

12. Executive power is defined according to the measure developed by Dodd (1976, 117),

$$(N_i[wa]/19)$$

where wa is the proportion (.001) of seats in the legislature of those parties whose addition to the existing cabinet would make the cabinet either minimum-winning or majoritarian or whose exclusion from the cabinet would render the cabinet minimum-winning or majoritarian. As there are more than one set of parties that can exist that will render the cabinet minimum-winning or majoritarian, we have followed the example of Dodd (1976, 118) and chosen the set whose wa is either largest (if the cabinet is oversized, i.e., more parties than necessary) or smallest (if the cabinet is minority status, i.e., too few parties to obtain a majority of seats in the legislature). N_i is the number of parties required to produce the necessary wa. The larger the value of the index, the less the power of the executive relative to the legislature (Dodd 1976; Lijphart 1984). Data for cabinets were taken from the authors' coding of cabinet formations as reported in *Keesing's Contemporary Archives* (1965–84), according to the definition of cabinet formation as employed by Dodd (1976) and Robertson (1984). The values of executive leadership ranged in our sample from 0 (the United States, Luxembourg, and New Zealand) to .259 (Switzerland). The mean value was .67 (standard deviation = .077). Having obtained these individual measures for the three separate indicators of issue disaggregation within the 20 democracies, we then derived separate, weighted, index scores for each democracy by factor-analyzing the data and obtaining standardized factor scores from this procedure. The larger the value of the factor score, the more the style of executive-legislative relations assumes the characteristics of the consensual model of democratic representation and departs from a style characteristic of the majoritarian model of democratic representation.

13. Following oblique rotation, one prominent dimension was identified with the following estimated commonalities for the respective three variables comprising the weighted index: dimensional polarization (.500), legislative fractionalization (.659), and executive power (.548). The factor scores derived from this analysis are very similar to those reported in Lijphart's (1984, 216) "effective number of parties" dimension. The mean value for the standardized factor score measuring the style of executive-legislative ratio is 0 (standard deviation = 1) with a minimum value of −1.645 (New Zealand) and a maximum value of 1.409 (Switzerland). While our index is designed to capture only the legislative-executive dimension of these democracies, our derived factor scores nonetheless correlate highly with Lijphart's (r = .855).

14. Representational volatility is defined according to the formula of Pederson (1983, 32–33),

$$.5(TNC_i)/19$$

where TNC is the total net change in party seat representation in a given legislature (i), as defined according to the formula

$$\sum_{i=1}^{n} | P_{iy} - P_{ij} = 1 |$$

where P_i is the proportion of seats (.001) held by party i in the legislature (j). Thus, the volatility of representation (RV_t) is the cumulated gains (or alternatively losses) in seats for all parties represented in the lower chamber of the national legislature. Data for (RV_t) are drawn from Mackie and Rose 1982 and *Keesing's Contemporary Archives* 1965–84 (for the later years). Data for (ΔU_t) are from OECD 1986.

15. The average annual volume of strike activity is the product of average annual frequency of strikes (number of strikes per 1,000 employees 1965–83), the average annual duration of strikes (number of days lost per strike 1965–83), and the average annual size of strikes (number of strikers per strike 1965–83). This measure follows that developed by Hibbs (1976, 1035–36) and employed by Cameron (1984, 176), Korpi (1983, 161–62), and Crouch (1985a). However, to remain consistent with the unit of analysis employed throughout this study, we have calculated WC_t as the product of the three components measured at the level of the broader time period (1965–83) rather than at the level of the individual year. Several of the countries do not have complete data for all three components during a given year, thus necessitating the computation of

WC_t at the broader time period level. Indeed, Luxembourg is deleted from this portion of the analysis because of insufficient and inequivalent data. Data were drawn from the International Labour Office 1965-85 (various volumes). Because of skewness in our sample, WC_t was log-transformed, a procedure employed by both Korpi (1983) and Crouch (1985a). Both the log-transformed data and the raw data correlate highly with the data on strike volume reported in Korpi (1983, 161-65), Cameron (1984, 153), and Crouch (1985a, 120). It is important to note that the International Labour Office data make no distinction between work stoppages and lockouts. The value of WC_t should therefore be properly considered as an indicator of class conflict, not merely employee strikes, or employer militancy (Crouch 1985a, 112-13).

16. It is possible to conceive that our results might be a function of specific time period effects, especially those associated with the relatively turbulent 1974-83 time period. In an analysis unreported here, we estimated the relationship between S_tE_t and ICR_tI_t on the one hand and PE_t and ΔPE_t on the other for those years. The results, which are available upon request, are consistent with the broader period. S_tE_t is negatively and statistically significantly correlated with PE_t, but positively and nonsignificantly correlated with ΔPE_t. ICR_tI_t is positively and significantly associated.

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THE RENAISSANCE OF POLITICAL CULTURE

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The publics of different societies are characterized by durable cultural orientations that have major political and economic consequences. Throughout the period from 1973 to 1987, given nationalities consistently showed relative high or low levels of a "civic culture"—a coherent syndrome of personal life satisfaction, political satisfaction, interpersonal trust and support for the existing social order. Those societies that rank high on this syndrome are much likelier to be stable democracies than those that rank low. Economic development and cultural change are linked in a complex pattern of reciprocal influence. Originally, Protestantism may have facilitated the rise of capitalism, leading to economic development, which in turn favored the emergence of the civic culture. But in those countries that attained high levels of prosperity, there eventually emerged postmaterialist values that tended to neutralize the emphasis on economic accumulation that earlier characterized Protestant societies.

It is time to redress the balance in social analysis. Since the late 1960s, rational choice models based on economic variables have become the dominant mode of analysis; while cultural factors have been deemphasized to an unrealistic degree. This approach has made major contributions to our understanding of how politics works; nevertheless, it underestimates the significance of cultural factors, if only because while economic indicators are readily available for these models, cultural data generally are not.

The incompleteness of models that ignore cultural factors is becoming increasingly evident. In Catholic societies from Latin America to Poland, the church plays a major role despite the demise often predicted by economic determinists. In the Islamic world, Muslim fundamentalism has become a political factor that neither East nor West can ignore. The Confucian-influenced zone of East Asia manifests an economic dynamism that outstrips any other region of the world. By economic criteria one of the *least*-favored regions on earth, it is virtually impossible to explain its performance without reference to cultural factors. Even in advanced industrial societies religion not only outweighs social class as an influence on electoral behavior (Lijphart 1979) but actually seems to be widening its lead: while social class voting has declined markedly in recent decades, religious cleavages remain astonishingly durable.

There is no question that economic factors are politically important, but they are only part of the story. I argue that different societies are characterized to very different degrees by a specific syndrome of political cultural attitudes; that these cultural differences are relatively enduring, but not immutable; and that they have major political consequences, being closely linked to the viability of democratic institutions.

After flourishing in the 1960s, the concept of political culture came under attack. In 1963 the fountainhead of political culture research, Almond and Verba's *The Civic Culture,* represented a tremendous advance. Previous works that attempted to deal with the impact of culture on politics relied on impressionistic evidence. Cultural influences on the distinctive political behavior of a given people were interpreted in terms of vague but presumably indelible characteristics such as "national character." By providing a well-developed theory of political culture based on cross-national empirical data, Almond and Verba moved from the realm of literary impressions to that of testable propositions.

In subsequent years, it was often charged that political culture was a static concept and that Almond and Verba had ethnocentrically asserted the (presumably permanent) superiority of Anglo-Saxon culture over that of other nations. For though their theoretical interests concerned possible changes in political culture, their analysis was based on data from a single time point and was therefore necessarily static. Empirically, the political culture of a given country could only be treated as a constant. The British and U.S. citizens were, as hypothesized, found to rank higher on interpersonal trust, pride in their political institutions, and feelings of political competence than the publics of Germany, Italy, or Mexico. But since these variables were in fact constants for each country, it was impossible to analyze their relationships with other macrophenomena or to trace changes over time.

The political culture literature argues that the evolution and persistence of mass-based democracy requires the emergence of certain supportive habits and attitudes among the general public. One of the most basic of these attitudes is a sense of interpersonal trust. Almond and Verba (1963) concluded that interpersonal trust is a prerequisite to the formation of secondary associations, which in turn is essential to effective political participation in any large democracy. A sense of trust is also required for the functioning of the democratic rules of the game: one must view the opposition as a *loyal* opposition, who will not imprison or execute you if you surrender political power but can be relied upon to govern within the laws and to surrender political power reciprocally if your side wins the next election. Almond and Verba found that their German and Italian respondents ranked relatively low on interpersonal trust. With data from only one time point it was impossible to determine whether these findings could be attributed to short-term factors—perhaps the harsh conditions of the post-war era—or whether they reflected more enduring differences. There was some reason to believe that the Italian findings, in particular, might reflect the heritage of long historical experiences (Banfield 1958).

The relationship between (1) a culture of distrust and (2) the presence or absence of modern social structures has the causal ambiguity of the chicken-versus-egg question: Does southern Europe have low levels of trust because it has not yet developed modern organizational structures? Or (in a variation on Weber's protestant ethic thesis) did southern Europe industrialize and develop modern organizational structures later than northern Europe because its traditional culture was relatively low on interpersonal trust? We cannot answer this question conclusively with the data now available. Banfield's interpretation implies that low levels of trust are a persisting feature of given cultures or regional subcultures, which may inhibit economic and political development in those areas. His critics emphasize the impact of economic development on cultural patterns. In our view a reciprocal causal relationship seems likely.

Important though it is, interpersonal

trust alone is not sufficient to support stable mass democracy. A long-term commitment to democratic institutions among the public is also required, in order to sustain democracy when conditions are dire. Even when democracy has no reply to the question, What have you done for me lately? it may be sustained by diffuse feelings that it is an inherently good thing. These feelings in turn may reflect economic and other successes that one experienced long ago or learned about second-hand as a part of one's early socialization. Evidence presented below indicates that the publics of certain societies have much more positive feeling toward the world they live in than do those of other societies. One of the best indicators of this orientation is satisfaction with one's life as a whole. This is a very diffuse attitude. It is not tied to the current performance of the economy or the authorities currently in office or to any specific aspect of society. Partly because it is so diffuse, intercultural differences in this orientation are remarkably enduring and may help shape attitudes toward more specific objects, such as the political system.

From their 1959 fieldwork, Almond and Verba found that (unlike the British or U.S. citizens) few Germans expressed pride in their political institutions. But one of the few aspects of their society in which they *did* express pride was the way their economic system was working. In the short run, this is an inadequate basis for democratic legitimacy. But in the long run, such feelings may contribute to the evolution of broadly favorable orientations toward the institutions under which one lives. Such feelings may play an important role in sustaining the viability of these institutions even when favorable economic or political outputs are not forthcoming. For cultural patterns, once established, possess considerable autonomy and can influence subsequent political and economic events. To demonstrate this fact, let us now turn to the analysis of data from cross-national surveys carried out during the past 15 years.

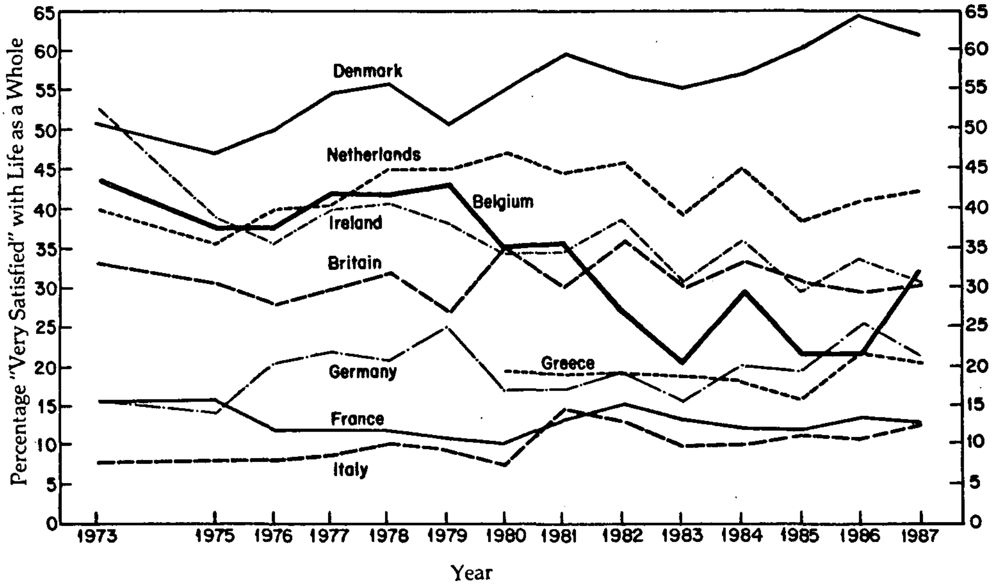
Cross-cultural Differences in Overall Life Satisfaction and Their Political Significance

The study of political culture is based on the implicit assumption that autonomous and reasonably enduring cross-cultural differences exist and that they can have important political consequences. Intuitively, these assumptions seem plausible. But critics of cultural explanations have questioned them, and indeed very little empirical evidence has been presented to support them so far. Since they are crucial assumptions underlying a controversial topic, let us examine a substantial body of evidence in order to see how well these assumptions hold up in longitudinal perspective.

I will start with one of the most basic and central attitudes of all: whether or not one is satisfied with the way things are going in one's life. Figure 1 illustrates the cross-national differences in response to the question, "Generally speaking, how satisfied are you with your life as a whole? Would you say you are very satisfied, fairly satisfied, not very satisfied, or not at all satisfied?" This question has been asked repeatedly in the Euro-Barometer surveys carried out from 1973 to the present. Figure 1 sums up the results from the over 200 thousand interviews in more than two hundred representative national surveys of the publics of nine European community nations.

I find large and remarkably stable cross-cultural differences. And these differences do not reflect objective economic conditions in any direct or simple fashion. Year after year the Italian public shows the lowest level of satisfaction; from 1973

Figure 1. Cross-national Differences in Levels of Satisfaction with Life, 1973–87



Source: Euro-Barometer surveys.

through 1987 they rank last in every year but one (when they rank second-lowest); at no time during this fourteen-year period do more than 15% of the Italian public describe themselves as "very satisfied." The French manifest only slightly higher levels of life satisfaction than the Italians, ranking in second-to-last place in all but two years (rising one rank above this level in one year and falling one rank below it in the other year). At no time do more than 17% of the French public describe themselves as "very satisfied."

At the opposite extreme, the Danes manifest the highest level of overall life satisfaction in every year but one (when they rank second); at no time do less than 47% of the Danish public describe themselves as "very satisfied." On the average, the Danes are six times as likely as the Italians to describe themselves as "very satisfied." The Dutch also rank high consistently, throughout the period from 1973 to 1987; at no time do less than 36% describe themselves as "very satisfied" with their lives as a whole.

The other nationalities maintain their relative positions in remarkably stable fashion, with a sole exception: the Belgians, who consistently ranked among the three most satisfied nationalities throughout the 1970s, show a substantial and protracted decline in the 1980s, falling to sixth place by 1986. In the 1970s, 40–45% of the Belgians consistently described themselves as "very satisfied"; in 1986, the figure had fallen to 21%. This drop of 15–20 points is not immense when compared with the gap of 50 points that separate the Danes from the Italians, but it does represent a substantial decline in the subjective well-being of the Belgian public and stands in dramatic contrast to the overall stability of the cross-national differences manifested throughout this period. The cultural differences are reasonably stable but not eternal. Short-term fluctuations are present and, as the Belgian case illustrates, significant changes can occur in the relative positions of given nations.

On the whole, the stability shown in Figure 1 is truly remarkable. For one must bear in mind that this was a period of sharp economic upheavals; the crises that occurred in the mid-1970s and the early 1980s constituted the two most severe recessions since the 1930s. Moreover, these crises had a much more severe impact on some societies than others. From 1973 to 1987, life satisfaction declined significantly in both Belgium and Ireland, two of the three countries most severely afflicted by unemployment and inflation during this period. Conversely, life satisfaction showed a modest but perceptible upward trend in Germany, the country with the *lowest* inflation rates from 1973 to 1987. Thus I find a fairly good fit between short-term fluctuations in life satisfaction and the economic experiences of the respective societies.

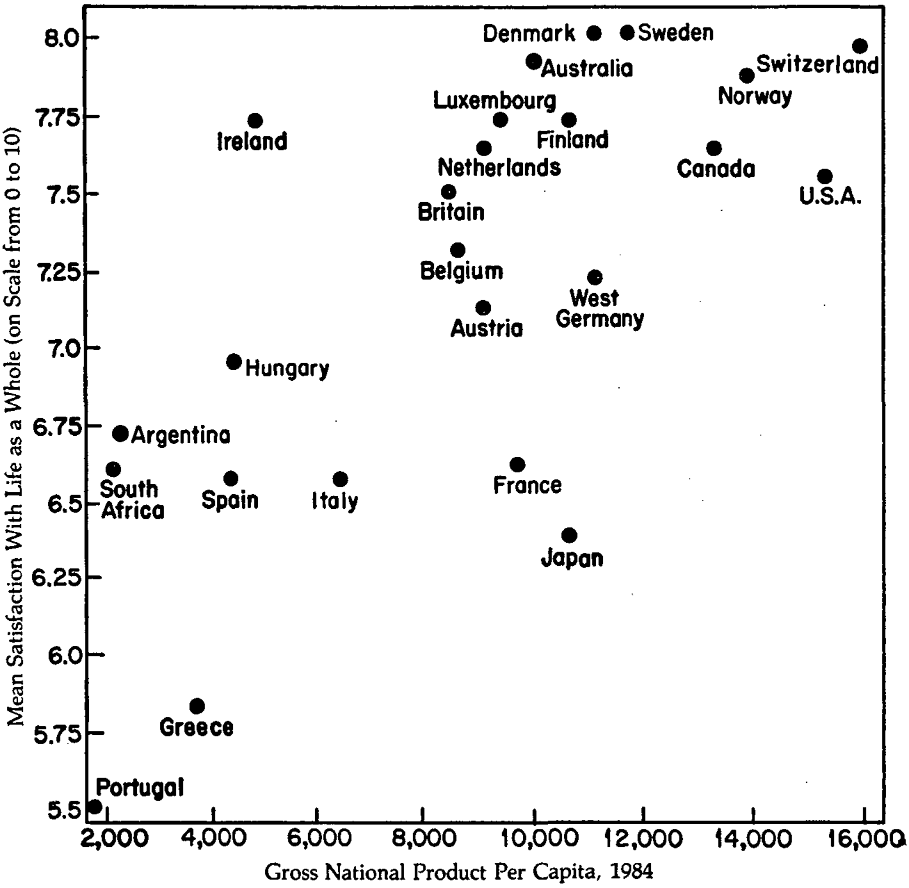
But a far more impressive aspect of Figure 1 is the cultural continuity that persisted *in spite of* these short-term fluctuations. Despite dramatic economic upheavals from year to year and despite large differences between the experiences of the different countries, I find remarkable stability in the relative positions of these publics. Through thick and thin, the Italians and French remain near the bottom and the Danes and Dutch near the top. And despite the fact that the German economy ranks high both in absolute terms and in relative performance, the life satisfaction levels of the German public consistently rank relatively low. Conversely, both the Irish and the Dutch have much lower per capita incomes than the Germans, and they experienced considerably higher levels of inflation and unemployment during this era than the Germans did, yet they continued to manifest higher levels of life satisfaction than the Germans. Though cross-cultural differences in life satisfaction respond to economic changes, they do so only with a great deal of inertia.

My conclusion is very simple but very important: there is a durable cultural component underlying these responses. Virtually any survey response is influenced to some extent by the context in which it is asked and this question is no exception: responses reflect both short-term fluctuations (resulting from immediate economic, social, and political events) and a long-term cultural component. Through statistical procedures it is possible to distinguish between the underlying cultural component and the short-term disturbances. In the present case, the long-term cultural differences are so pronounced that one can readily perceive them by mere visual inspection. Enduring cross-cultural differences exist and can be measured.[1]

I suggest that the cultural component of these cross-national differences reflects the distinctive historical experience of the respective nationalities. Long periods of disappointed expectations give rise to dissatisfied attitudes. These orientations may be transmitted from generation to generation through preadult socialization. In so far as early learning is relatively persistent, this contributes to the stability of distinctive cultural patterns. The fact that one can to some extent identify the historical causes of given cross-cultural differences does not of course make them disappear. They remain distinctive cultural characteristics with important behavioral consequences.

Is it true that economic security tends to enhance the prevailing sense of life satisfaction in a society, gradually giving rise to a relatively high cultural norm? Empirical evidence supports this supposition. First, as we have seen, there is a tendency for life satisfaction levels to rise or decline gradually in response to short-term economic fluctuations. But one

Figure 2. Mass Life Satisfaction, by Level of Economic Development



Note: $r = .67$.

might suspect that the observed cross-cultural differences reflect long-term historical experiences over generations or even centuries, not just the past dozen years or so. We would need survey data covering the past century or two in order to test this hypothesis directly. They are not available, but we can use the cross-sectional pattern to provide a surrogate test: if economic security is conducive to relatively high levels of life satisfaction, we would expect the publics of prosperous nations to show higher levels of satisfaction than those of poorer ones. The data in Figure 2 tests this hypothesis.

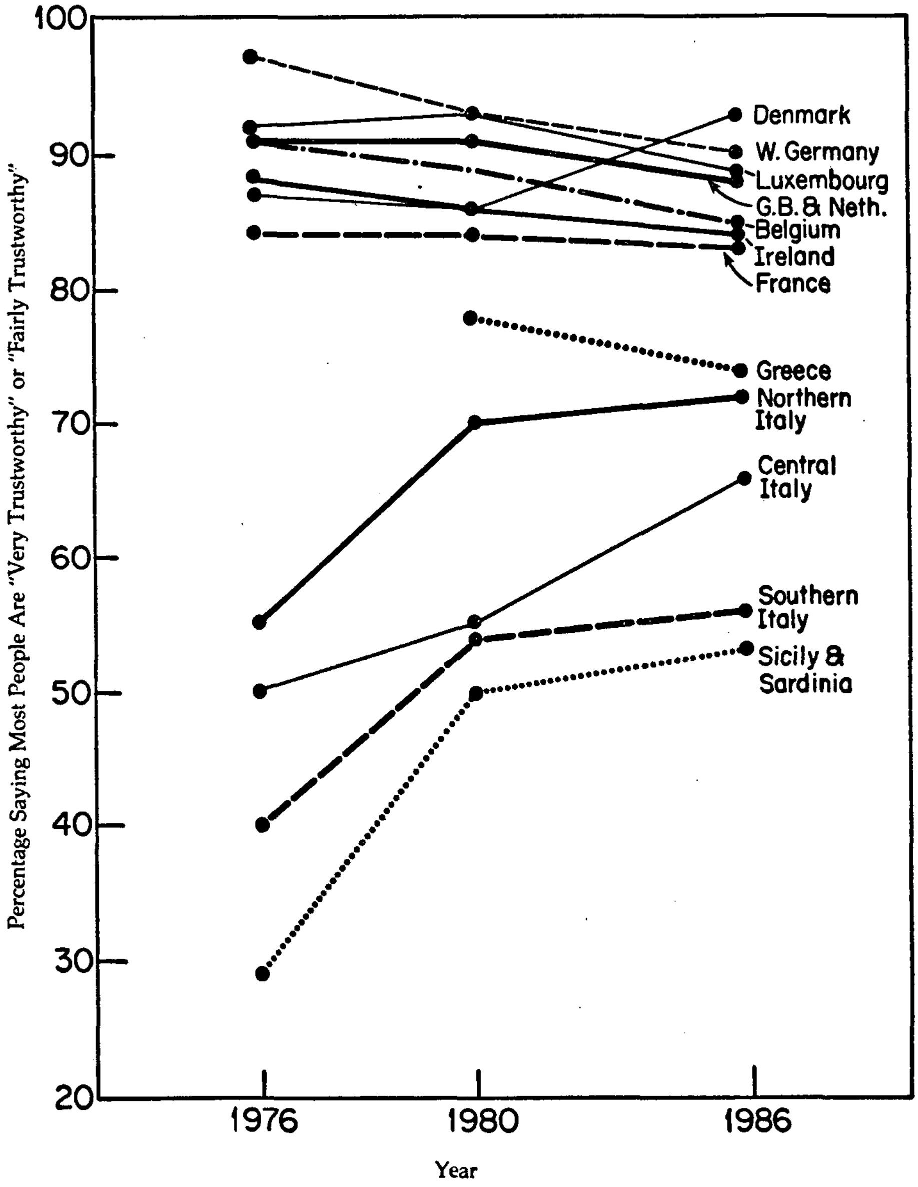
The overall correlation between gross national product per capita and life satisfaction in Figure 2 is .67: prosperity *is* linked with relatively high levels of life satisfaction among the 24 nations for which we have data from the 1980s. This point has been a matter of controversy in previous studies. Cantril (1965) analyzed data gathered in the 1950s from 14 countries and found that the publics of richer nations *did* show relatively high levels of subjective well-being. Easterlin (1974) reanalyzed the Cantril data and concluded that the correlation was actually rather weak. Emphasizing the fact that some poor nations (such as Egypt) showed higher levels of life satisfaction than some relatively wealthy ones (such as West Germany), he argued that economic development had little impact on subjective well-being. In a more recent study based on a broader range of nations Gallup (1976) found a relatively strong correlation between economic development and life satisfaction. He concluded that the two *are* linked. The present data also show a relatively strong correlation between economic development and life satisfaction. How we interpret this depends on one's theoretical expectations. If one approaches the topic with the expectation that subjective well-being will be found almost entirely a matter of economic factors, as Easterlin apparently did, then the crucial finding is that this is not the case. With the data in Figure 2 a nation's economic level explains less than half of the variance in life satisfaction. In the data Easterlin analyzed economic factors explain an even smaller share of the variance. From Easterlin's perspective, economic determinism was clearly discredited.

However, if one approaches the question with the expectation that a nation's level of economic development is only one of a number of historical factors that influence cross-cultural differences in life satisfaction, then the data clearly do support the hypothesis. Here, as in Easterlin's analysis, one can point to some striking deviant cases. Nevertheless, the overall pattern is clear: wealthier nations tend to show higher levels of life satisfaction than poorer ones.

The Euro-Barometer surveys also provide regular readings on political satisfaction, which shows much more short-term fluctuation than life satisfaction, for it explicitly refers to the political system and accordingly behaves like an indicator of governmental popularity, fluctuating from one month to the next in response to current economic conditions and political events. When a conservative government is in office, those who identify with the Right show higher levels of political satisfaction; when a government of the Left gains power, those on the Left show higher levels. Political satisfaction fluctuates in response to current economic and political events. But it is clear that a significant cultural component *also* is present underneath these fluctuations: the publics of some countries are consistently more satisfied than others. Moreover, these differences reflect the now-familiar pattern found with life satisfaction and happiness: the publics of France and Italy almost always rank lower on political satisfaction than those of other nations: at the national level, the correlation between life satisfaction and political satisfaction is .41.

Both overall life satisfaction and political satisfaction are correlated with stable democracy. I will examine this relationship below. For the moment let me simply note that political satisfaction levels are only weakly linked with the number of years that democratic institutions have persisted in a given nation (r = .21). But *despite* the direct and obvious political relevance of political satisfaction, life satisfaction is far more strongly linked with stable democracy (r = .85). Democratic institutions seem to depend on enduring cultural traits such as life satisfaction and interpersonal trust, more than on relatively fluctuating variables such as political satisfaction. The latter may well be a better predictor of the popularity of a given government at a given moment. But precisely *because* it fluctuates with short-term conditions, it is less effective in maintaining the long-term stability of democratic institutions.

Figure 3. Interpersonal Trust among European Publics, 1976–86



Source: Data from Euro-Barometer surveys 6, 14, and 25.

Interpersonal Trust, Economic Development, and Democracy

Following Banfield, Almond and Verba, Wylie, and others, I hypothesized that interpersonal trust is part of an enduring cultural syndrome that is conducive to the viability of democracy. The first question one must answer is, Do enduring intercultural differences exist in interpersonal trust? Unless they do, any argument concerning its long-term political impact is on shaky ground.

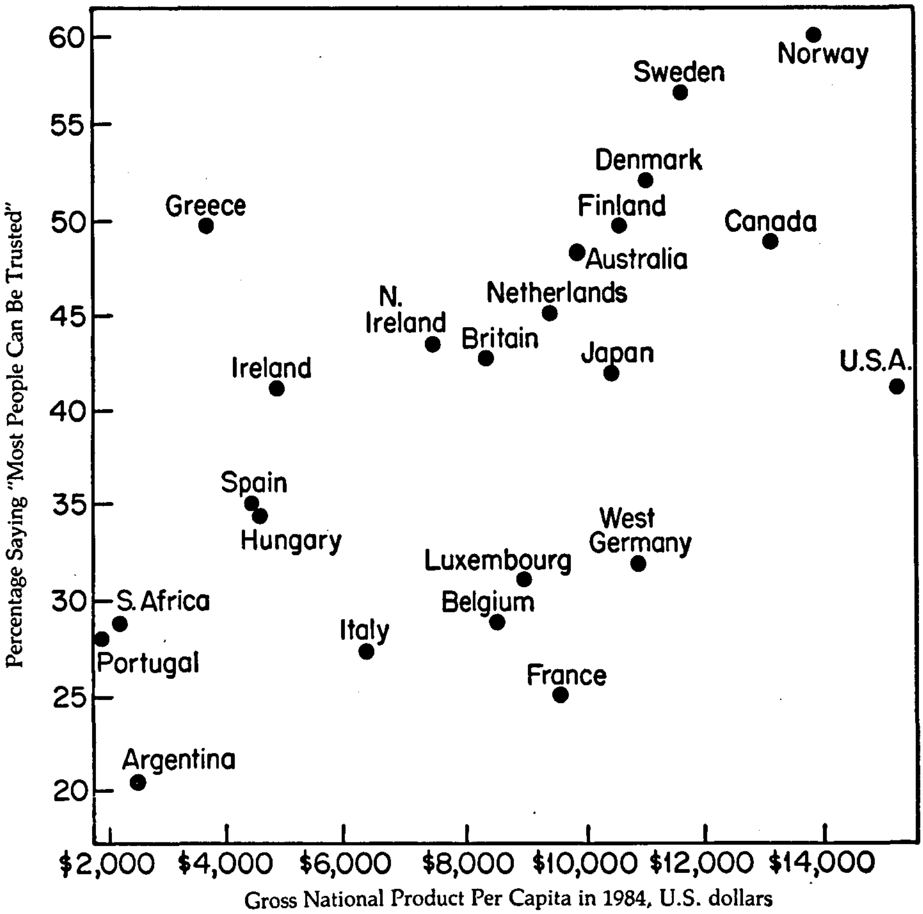
The evidence indicates that given societies are indeed characterized by distinctive levels of interpersonal trust. This even seems to be true of specific regions *within* given countries. Euro-Barometer surveys in 1976, 1980 and 1986 asked, "Now I would like to ask about how much you would trust people from various countries. For each country, please say whether, in your opinion, they are generally very trustworthy, fairly trustworthy, not particularly trustworthy, or not at all trustworthy."

Figure 3 depicts the relative levels of interpersonal trust expressed during this 10-year period toward people of one's *own* nationality. As is immediately evident, trust levels in given countries are extremely stable. In a pattern that is becoming increasingly familiar, the Italian public ranks lowest at every point in time (with four major regions of Italy each retaining its relative position). The Greeks rank next, followed by the French. The other nationalities are clustered close together, with relative rankings consistently remaining within the band between 85% and 95%. One line in Figure 3, labeled "G.B. & Neth.," depicts the levels of *both* the British and the Dutch, which are identical at all three points. Remarkably pronounced and durable differences in trust exist among the various regions of Italy. Precisely as Banfield (1958) found many years ago, southern Italy seems to be characterized by lower levels of interpersonal trust than northern or central Italy and than any other Western society for which we have data.

These findings of stable regional differences in Italian political culture are consistent with earlier findings by Putnam and his colleagues (1983) based on ecological data covering a much longer period of time. In an imaginative and elegant analysis, these authors utilize various indicators of social involvement and political mobilization (such as membership in mutual aid societies, union membership, and electoral turnout) to derive a measure of "civic culture." This variable manifests remarkable stability at the regional level: their index of civic culture as measured in the 1970s correlates at $r = .91$ with the strength of mass parties in 1919–21 and at $r = .84$ with the strength of mutual aid societies from 1873 to 1904. Civic culture in turn proves to be strongly correlated with the relative success of the new regional governments established throughout Italy in 1970. The authors conclude that political success or failure largely reflects the impact of autonomous cultural factors, independent of underlying economic variables:

> Contrary to any simple-minded economic determinism, these regional continuities in political culture are strikingly greater than continuities in economic structure or social well-being. For example, the agricultural share of the regional work force in 1970 correlated $r = -.02$ with the same figure in 1870; the equivalent statistic for infant mortality is $r = .01$. Those regions with a relatively agricultural economy in 1970 had not been the more agricultural regions a century earlier, and the regions with good public health in 1970 had not been the healthier ones in 1870. But the regions characterized by political activism and social solidarity in the 1970s were essentially the same regions that had been so a century earlier. In short, we can trace with remarkable fidelity over the last hundred years the historical antecedents of just those aspects of regional political culture—mass participation and civic solidarity—that in turn provide such a powerful explanation for contemporary institutional success." (Putnam et al. 1983, 69–70)

Figure 4. Economic Development and Interpersonal Trust



Sources: Data from World Values Survey, 1981 and for Greece, Portugal and Luxembourg Euro-Barometer Survey 25 (April 1986).

Note: $r = .53$.

Despite evidence of impressive stability in the propensity to trust others, trust is not a fixed genetic characteristic: it is cultural, shaped by the historical experiences of given peoples and subject to change. Fieldwork carried out for *The Civic Culture* found that the publics of the two long-established English-speaking democracies had markedly higher levels of interpersonal trust than those of Mexico, West Germany, or Italy. The Italian public, in particular, manifested trust levels that were phenomenally low in 1959. But as our theoretical framework implies, the economic miracles that took place in both West Germany and Italy during the 1950s and 1960s eventually had an impact on the political culture of these countries. Though the Italian public still remained relatively low on trust in 1981 and 1986, absolute levels of trust had more than tripled by 1981 and almost

Table 1. Support for Radical Change, Gradual Reform, or Defense of Present Society, in 20 Societies, 1981 (%)

Society	Radical Change[a]	Gradual Reform[b]	Defend Present Society[c]	Number of Cases
N. Ireland	1	74	25	287
Norway	2	50	49	1,146
W. Germany	3	59	38	1,149
Denmark	3	69	28	1,026
Japan	3	71	26	707
Netherlands	3	70	26	1,063
Sweden	4	80	16	797
Ireland	4	76	20	1,084
Luxembourg	5	69	25	760
Canada	5	74	21	1,135
Britain	5	73	23	1,136
U.S.A.	5	73	22	2,101
Belgium	7	73	21	897
Italy	8	73	19	1,270
Spain	8	82	10	2,091
Greece	9	57	26	2,000
France	9	73	19	1,123
Mexico	12	77	11	1,610
Portugal	14	74	12	813
S. Africa	25	54	21	1,182

Sources: World Values survey, 1981; except that data for Greece and Luxembourg are from Euro-Barometers 15 and 16 (carried out in April and October 1981), and Portuguese data are from Euro-Barometer 24 (carried out in November 1985). These are the earliest such data available from Portugal.

[a] "The entire way our society is organized must be radically changed by revolutionary action."

[b] "Our society must be gradually improved by reforms."

[c] "Our present society must be valiantly defended against all subversive forces."

are reasonably strong grounds for thinking that while it may work both ways, the former of the two processes is mainly responsible for the relationship. First, knowing the relatively modest place that politics has on most people's intellectual horizons, it seems far more plausible that people vote Communist or neo-Fascist because they are dissatisfied and distrustful than that they are dissatisfied and distrustful because they vote Communist (or as a right-wing fantasy might have it, that communism spreads unhappiness and distrust among the public). Secondly, low levels of trust, satisfaction, and happiness are broad cultural characteristics of the French, Greek, and Italian publics, only marginally more prevalent among the electorates of the Communists and extreme Right than elsewhere. If the Communists sow unhappiness, they sow it broadly, not just among their own supporters. Finally, there are indications that gradual cultural quadrupled by 1986—a phenomenon that parallels the rise in Italy shown in Figure 3. The growth of interpersonal trust among the West German public was proportionately smaller, because they started from a higher level. But by 1986 the Germans had actually surpassed the British in interpersonal trust. The other side of the coin is the fact that interpersonal trust had shown a long-term decline in the two English-speaking democracies.

Like life satisfaction and happiness, high interpersonal trust goes with relatively high levels of economic development, as Figure 4 illustrates. The cross-sectional correlations is .53. The available data do not enable one to determine whether this is because interpersonal trust is conducive to economic development or because economic development leads to an enhanced sense of security that is conducive to trust or whether (as we suspect) the two processes are mutually supportive. It is interesting that in the two countries from which I have evidence of a dramatic rise in interpersonal trust (West Germany and Italy) this phenomenon took place after a period of dramatic economic recovery. But it is clear that economic factors alone are not decisive; for the publics of both Britain and the United States were wealthier in the 1980s than they were in 1959–60, but both experienced an erosion of interpersonal trust from 1960 to 1981.

As we will see shortly, high levels of interpersonal trust are also linked with stable democracy. But before analyzing this relationship, let us consider the implications of a recurrent finding in the data. With remarkable consistency, the publics of France and Italy rank lowest on the syndrome of attitudes that might loosely be called the civic culture. Among those societies for which time series data from 1973 to the present exist, the French and Italian publics nearly always rank lowest on life satisfaction, happiness, political satisfaction, and trust. And these are precisely the countries that have been characterized by the largest antisystem parties: in France since the end of World War I the Communists have normally polled a fifth or more of the total vote in national elections. More recently, a party of the extreme Right, the National Front, won about 10% of the vote in nationwide elections in 1984 and again in 1986, recalling the sudden mushrooming of support for another extreme Right party, the Poujadists, that took place in the 1950s. Similarly, in Italy the Communists have generally won about a third of the vote since 1945, while the neo-Fascist Italian Social Movement has often won as much as 10% of the vote in national elections.

Data from Greece are available only since 1980, but they fit the pattern just described. To be sure, the Greek public has shown relatively high levels of satisfaction with the way democracy is functioning since democratic institutions were restored in the 1970s. But as I have already noted, this variable fluctuates with short-term events. On most of the long-term civic culture indicators such as life satisfaction, happiness, and support for the existing social order, the Greek public ranks relatively low. In keeping with this pattern, support for antisystem parties has been relatively high in Greece. The Communist vote has been higher than in any of the other societies for which time series data exist, apart from France and Italy; and there has also been significant support for parties of the extreme Right.

I believe that relatively low levels of diffuse satisfaction and trust make one more likely to reject the existing political system and support parties of the extreme Right or Left. Again, we have the chicken-versus-egg question: Does a culture of dissatisfaction and distrust give rise to an extremist vote or do extremist parties produce distrust and dissatisfaction? The available empirical data do not allow me to provide a conclusive answer; but there change *precedes* changes in the vote. Table 1 shows the cross-national pattern of responses to the following question:

> On this card (*show card*) are three basic kinds of attitudes concerning the kind of society we live in. Please choose the one which best describes your own opinion.
>
> 1. The entire way our society is organized must be radically changed by revolutionary action.
> 2. Our society must be gradually improved by reforms.
> 3. Our present society must be valiantly defended against all subversive forces.

As Table 1 demonstrates, there is wide cross-national variation in response to this question. In the early 1980s, support for the revolutionary option ranged from a low of 1% or 2% in Northern Ireland and Norway to highs of 12%-25% in Mexico, Portugal, and South Africa. Conversely, support for defense of the status quo ranged from highs of 49% and 38% respectively in Norway and West Germany to lows of 10%-12% in Mexico, Spain, and Portugal. The revolutionary option was most likely to be endorsed in societies with a relatively *low* per capita GNP ($r = -.68$), while the conservative option was most likely to be endorsed in societies with a relatively high per capita GNP ($r = .58$). And, as one might expect, support for the revolutionary option is negatively correlated with life satisfaction ($r = -.52$); while support for the conservative option shows a positive correlation with life satisfaction ($r = .31$).

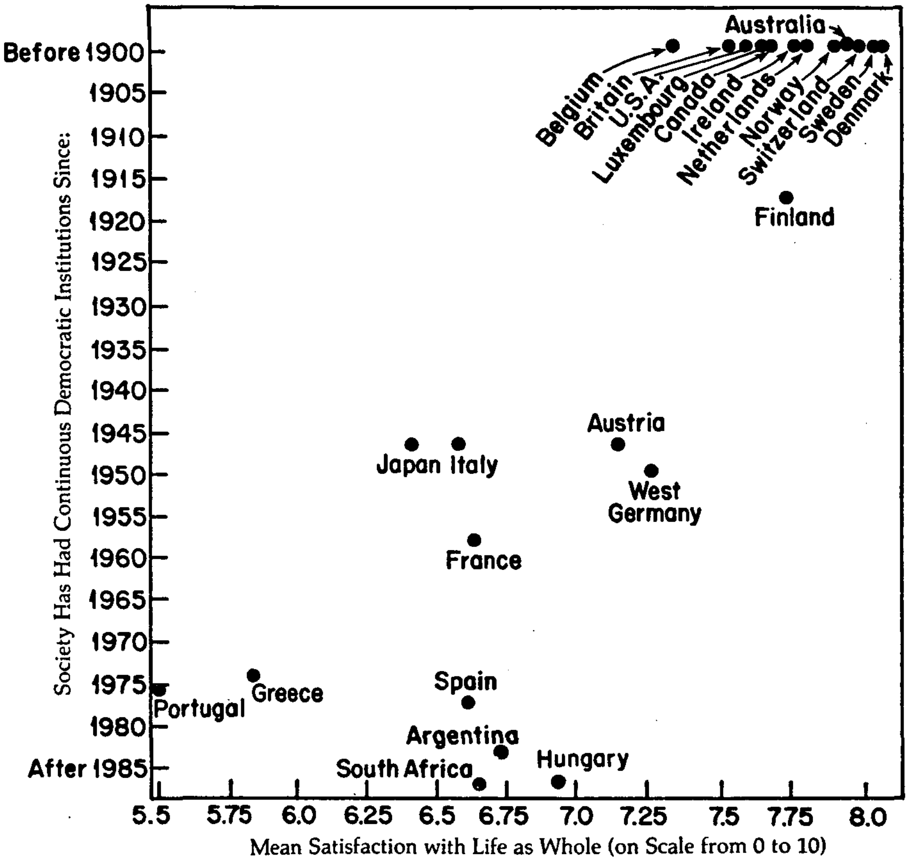
Responses to this question among the nine European Community nations from which we have data from 1976 through 1986 are quite stable, apart from a gradual decline in support for the revolutionary option. In 1976 it was supported by 9% of the public in the European Community as a whole but in 1986 by only 5%. This decline was gradual, never falling by more than one percentage point per year and was pervasive, with most nations showing declining support for revolution. But the phenomenon was especially concentrated among the publics of France and Italy, where prorevolutionaries constituted 14% of the public in both countries in 1976 but only 7% and 8% respectively in 1986. In part, this reflects the deradicalization of the Communist electorates of France and Italy, which *preceded* the electoral decline experienced in the mid-1980s by the Communist parties of these two countries (and to some extent, throughout Western Europe). Moreover, it may be a harbinger of favorable prospects for the persistence of democratic institutions in these countries, for my data show a correlation of −.73 between support for the revolutionary option, and the number of continuous years that democratic institutions have functioned in a given nation.

The Consequences of Political Culture: Some Speculations with Data

Let us sum up what we have learned so far. I find a broad syndrome of related attitudes that show substantial and consistent cross-cultural variation, with certain societies being characterized by satisfied and trusting attitudes to a much greater degree than others. The cross-national differences show impressive stability over time. Though they can vary (and the variations are of great substantive interest), they tend to be relatively enduring cultural characteristics. Finally, this syndrome is linked with the persistance of democratic institutions.

Life satisfaction, political satisfaction, interpersonal trust, and support for the existing social order all tend to go together. They constitute a syndrome of positive attitudes toward the world one lives in. And this syndrome goes with enduring democratic institutions. Is the linkage a causal one? Such causal linkages are difficult to demonstrate conclusively. To do so would require political culture data from a large number of nations, some of which became democracies during the course of a long time series, while others did not. My interpretation implies that those societies characterized by high levels of life satisfaction (as well as interpersonal trust, tolerance, etc.) would be likelier to adopt and maintain democratic institutions than those whose publics lacked such attitudes. Conversely, democratic institutions would be more likely to flounder in societies with low levels of life satisfaction, trust, and so on.

Figure 5. Mass Life Satisfaction and Stable Democracy



Sources: Data from World Values survey and Euro-Barometer survey.

Note: $r = .85$.

Such data are not now available and will be difficult to obtain, both because it will require a long-term data-gathering process in many countries during many decades and because the governments of nondemocratic societies usually make it difficult to carry out survey research. In principle, however, it is possible to acquire such data, and this is a goal worth striving for. We may not attain the optimum, but we can certainly improve on what we have now. In the meantime, let us examine the cross-national pattern: do democratic institutions seem to have emerged earlier and persisted longer in societies with high levels of overall life satisfaction, than in those characterized by relatively low levels?

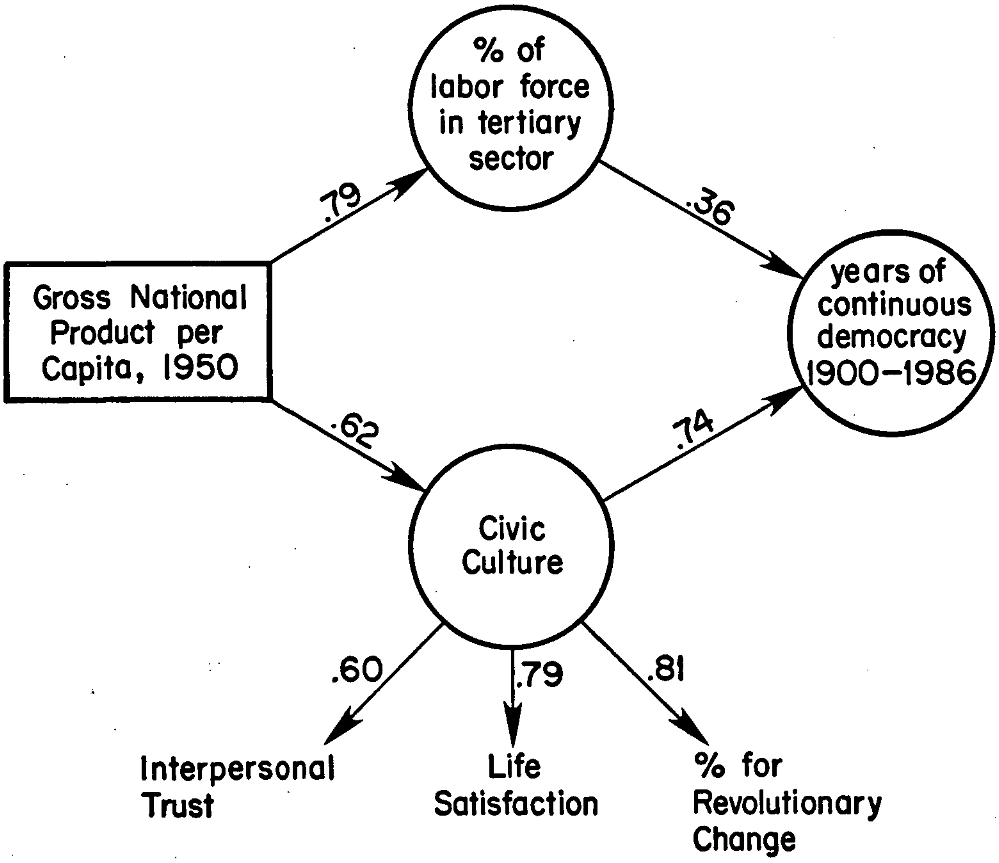
As Figure 5 demonstrates, the answer is *yes.* There is a remarkably consistent tendency for high levels of life satisfaction to go together with the persistence of democratic institutions over relatively long periods of time. Among the 24 societies depicted in Figure 5, the overall correlation between life satisfaction and the number of continuous years a given society has functioned as a democracy is .85. Needless to say, the causal inference would be on firmer ground if I had survey data on life satisfaction levels from some much earlier point in time, such as 1900, but such data is not available. I use ex post facto data from 1981 as an indicator of the relative rankings earlier in history. The evidence indicates that these rankings are pretty stable, but this procedure undoubtedly introduces some error in measurement (which tends to work *against* my hypothesis). Since our focus is on the effects of domestic political culture, I code democracy as having failed to survive only when it collapsed through *internal* causes and not when it did so as a consequence of foreign conquest. By this definition, literally *all* of the 13 societies that have maintained democratic institutions continuously since 1920 or earlier, show relatively high life satisfaction levels (above 7.25 on a scale from 0 to 10). Among the 11 societies in which democratic institutions have emerged only since 1945 or which are not yet fully democratic all but one show mean life satisfaction levels below 7.25 and in some cases far below it. Some data are also available from Mexico, but because they are not based on a representative national sample, they have been omitted from these quantitative cross-national analyses. For what they are worth, these data show surprisingly high levels of life satisfaction, paralleling earlier findings by Almond and Verba that the Mexican public showed anomalously high levels of satisfaction with their political system despite relatively negative conditions.

The coding of France might seem questionable. It is coded as having continuous democratic institutions only since 1958, since the military uprising that brought the Fourth Republic to an end and brought DeGaulle to power was not of foreign origin but was carried out by the French Army. It is true that the last president of the Fourth Republic invited DeGaulle to form a government. But he did so only under pressure from the French Army. Free elections were held shortly afterward, so the suspension of democracy was very brief; but as the subsequent military uprising of 1960 and 1962 testify, democracy in France was for a time on shaky ground.

It is conceivable that we have the causal arrow reversed. Perhaps many decades of living under democratic institutions produces greater life satisfaction. We don't rule this factor out. Indeed, we think it *does* contribute to overall life satisfaction somewhat. But theoretical considerations suggest that the process mainly works the other way around. It seems more likely that a global sense of well-being would also shape one's attitudes toward politics than that what is experienced in one relatively narrow aspect of life would determine one's overall sense of satisfaction. In keeping with this reasoning, Andrews and Withey (1976) find that political satisfaction has only a relatively modest impact on most people's overall life satisfaction; satisfaction with one's job, home, family life, and leisure time all make larger contributions.

Overall life satisfaction is part of a broad syndrome of attitudes reflecting whether one has relatively positive or negative attitudes toward the world in which one lives. Life satisfaction, happiness, interpersonal trust, and whether one supports radical social change or defends one's existing society, all tend to go together in a cultural cluster that is closely related to whether or not democratic institutions have persisted for a long time in a given society. These attitudes seem to be a deep-seated aspect of given cultures, constituting a long-term component underlying absolute levels of satisfaction with governmental performance at any given time. Though political

Figure 6. Economic and Cultural Prerequisites of Stable Democracy


% of labor force in tertiary sector
Gross National Product per Capita, 1950
years of continuous democracy 1900–1986
Civic Culture
.79
.36
.62
.74
.60
.79
.81
Interpersonal Trust
Life Satisfaction
% for Revolutionary Change

Source: LISREL analysis of data from the societies shown in Figure 5.

Note: Adjusted goodness of fit index = .88.

satisfaction shows sharp fluctuations from one month to the next, the publics of some societies consistently manifest higher levels of satisfaction than the publics of others.

This syndrome is also linked with a society's economic level. The more developed societies tend to rank relatively high on life satisfaction, trust, and the other components of the syndrome. Are both it and liberal democracy simply joint consequences of economic development or does political culture make an autonomous contribution to the viability of democratic institutions, as Almond and Verba have argued? Let me emphasize that we do not yet possess a data base that would enable us to answer such questions conclusively. We have established the presence of an enduring and cross-nationally distinctive syndrome of basic cultural attitudes and demonstrated that this syndrome is much stronger in those societies that have been stable democracies since 1900 than in those that have been nondemocratic or intermittently democratic. But we do not yet have sufficient data to sort out the causal linkages between political culture, economic development, and democracy in any conclusive fashion. To do so would require regular measures of the relevant cultural variables throughout the past century. At present, they are simply not available. Nevertheless, the patterns shown in Figures 2, 4, and 5 are so striking and their implications are so important that they virtually cry out for further exploration. The remainder of this article will be devoted to some analyses that cannot be conclusive, but which suggest that cultural factors may play an extremely significant role in both political and economic development. I hope these analyses may stimulate further historical analysis and the development of the longitudinal data base necessary for any definitive analysis of the role of culture.

Figure 6 shows the results of a LISREL analysis in which the dependent variable is the number of consecutive years since 1900 that democracy has functioned in a given society. The results indicate that economic development per se does *not* necessarily lead to democracy. Only in so far as it brings appropriate changes in social structure and political culture does it enhance the viability of democratic institutions. To take an extreme illustration, such nations as Saudi Arabia, Kuwait, and Lybia are quite wealthy, but neither their social structures nor their political cultures seem favorable to democracy.

Clearly, I would prefer to have data gathered at earlier time points for some of my variables, especially the cultural indicators. I use gross national product per capita in 1950 as an indicator of a given nation's level of economic development, this being the earliest time for which I have reliable data from all 24 societies. Theoretically, an even earlier time point might better tap the impact of economic development on the growth of the tertiary sector. However, the 1950 data were measured about 30 years before the political culture indicators (based on 1981 surveys), providing a time-lag of about one generation between economic and cultural indicators, which is appropriate for present purposes.

The results suggest that political culture is a crucial link between economic development and democracy. Although GNP per capita has a strong zero-order correlation with stable democracy, its effects are almost entirely due to its linkages with social structure and political culture, with the latter being the more important of the two. Earlier versions of the LISREL model allowed for a direct linkage between economic development and democracy, but its empirical importance was so insignificant ($r = .08$) that the final model shown in Figure 6 omits it. A given nation's level of economic development is closely linked with a set of characteristics that we label the "civic culture" (though it is only roughly equivalent to Almond and Verba's construct). This political culture syndrome is tapped by three indicators: (1) interpersonal trust, (2) life satisfaction, and (3) support for revolutionary change, the latter being negatively correlated with the "civic culture." All three variables are good indicators of this underlying cultural dimension, with the second and third indicators showing almost identical correlations, despite the fact that in face content, overall life satisfaction has no obvious relationship to politics, while support for revolutionary change clearly does. Earlier versions of the model also included a fourth indicator of the civic culture, satisfaction with the way democracy is functioning in one's country. Though in face content this variable has the most obvious reference to democratic institutions, it manifests a relatively large amount of short-term fluctuation. This variable seems to be a better indicator of governmental popularity at a given time than of long-term support for democracy. And though it has a significant correlation with the civic culture dimension ($r = .54$), it taps it less accurately than the other three indicators. The same is true of a given public's rate of political discussion. With the small number of cases at my disposal, I obtain a better fitting model by omitting these variables.

The underlying "civic culture" tapped by these three indicators shows a strong linkage with the number of years that democratic institutions have functioned in the given society: the regression coefficient is .74, controlling for the effects of social structure. This suggests that over half of the variance in the persistence of democratic institutions can be attributed to the effects of political culture alone.

The size of the tertiary sector also has a significant linkage with the persistence of democracy. A measure of the relative size of the middle class as opposed to those with blue collar or agricultural occupations, it is also an indirect indicator of the strength of commercial elites. The importance of this variable provides empirical support for the arguments of theorists from Marx to Lipset, who have emphasized the significance of the development of a commercial-industrial middle class. In part this variable reflects purely economic factors: a less polarized distribution of income and the presence of a middle majority tend to make political conflict less ruthless and less desperate. But historically, democratic institutions emerged long *before* the redistributive welfare state and the middle majority: democracy emerged when power passed into the hands of the bourgeoisie. Moore (1966) takes it as axiomatic that there is an inherent affinity between democracy and the bourgeoisie. But if one presses farther and inquires *why* this is so, one is likely to arrive at a cultural explanation as a crucial intervening variable. Preindustrial agrarian societies are typically dominated by landed aristocracies exercising a military function or by priestly elites, both of which are accustomed to social relations based on a hierarchical chain of command structure. Commercial elites, by contrast, necessarily become accustomed to bargaining. In market relationships the buyer and seller may have diametrically opposed interests, the seller seeking the highest possible price and the buyer the lowest, but unless they can reach a compromise that is acceptable to both sides, neither can do business.

It seems clear that viable democracy does *not* depend on economic factors alone. Specific cultural conditions are crucial, and they in turn are related to economic and macropolitical developments. Long-term economic success can help provide legitimacy for any type of regime in industrial society. Thus it can help maintain the viability of democratic institutions once they are established. But unless economic development is accompanied by certain changes in social structure and political culture, liberal democracy is unlikely to result. Moreover, external constraints can prevent the emergence of democracy even when internal factors are favorable. Huntington (1984) argues that Czechoslovakia and (probably) Poland and Hungary have social and cultural conditions that would favor increasing democratization, in the absence of direct or indirect Soviet intervention. To what degree various East European societies now have political cultures that would support democratic institutions if the Soviet Union permitted them to emerge is a fascinating but unanswered question. It would be as gross an oversimplication to believe in sheer cultural determinism as to believe in economic determinism. Stable democracy reflects the interaction of economic, political, *and* cultural factors. While economic development does not automatically bring about democracy, it does seem to be linked with sociocultural changes that enhance its chances.

Cultural Change and Economic Development

I have established that certain societies are characterized relatively strongly by a durable set of orientations that roughly corresponds to the "civic culture" discussed by Almond and Verba and that this cultural pattern shows a strong empirical linkage with stable democracy even when I control for related aspects of social structure and economic development. In other words, a body of evidence that not only is much larger than that available to Almond and Verba but also extends over a number of years tends to confirm the basic thesis of *The Civic Culture*.

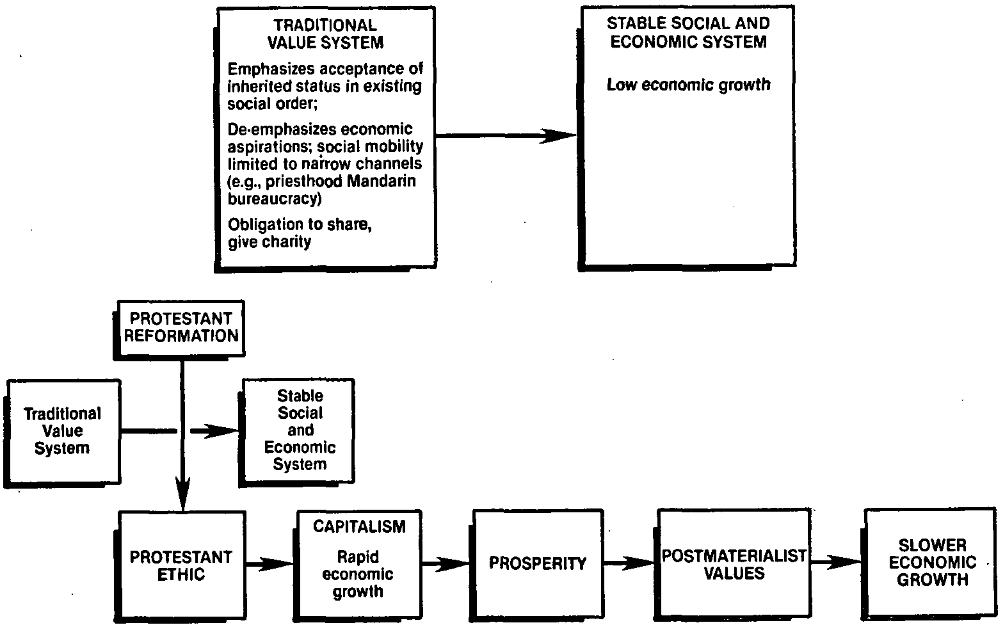
But the civic culture itself is only one aspect of a still broader cultural syndrome that seems to reflect a long-term process of social and economic change. To understand this process, one must see it in the context of historical developments that have taken place over the past few centuries. Though we do not have survey data that would enable us to trace directly the long-term cultural changes linked with this process, some available indicators enable us at least to suggest the broad outlines of what has been happening.

Max Weber (1958) argued at the turn of the century that the rise of capitalism and the subsequent rapid economic development of the West were made possible by a set of cultural changes related to the emergence of Calvinist Protestantism. His Protestant ethic thesis gave rise to a controversy that endured for decades. Some of the criticisms seem well founded; and the thesis that economic achievement was linked with Protestantism may seem unconvincing today, when predominantly Catholic countries have higher economic growth rates than Protestant ones. Nevertheless, though I would not defend Weber's thesis in its entirety, I believe that important aspects of it were correct, provided his work is viewed as an analysis of a specific historical phenomenon (as was clearly Weber's intention) and not as asserting an immutable relationship between economic achievement and Protestantism. Particularly crucial is Weber's insight that culture is not simply an epiphenomenon determined by economics but an autonomous set of factors that sometimes shape economic events as well as being shaped by them.

I utilize the dominant religious tradition of a given society as an indicator of its preindustrial cultural heritage. This is, of course, an oversimplified indicator, though not as oversimplified as it may seem from today's perspective. Contemporary social scientists tend to underestimate the historical importance of religion, both because they are social scientists, habituated to viewing the world from a secular and scientific viewpoint, and because they are contemporary and live in societies in which the functions of religion have diminished drastically. In most agrarian societies, religion is an overwhelmingly important force, filling the functions that educational and scientific institutions, the mass media, art museums, and philanthropic foundations, as well as religious institutions, now fill in advanced industrial societies. In modern societies, religion is a far less adequate indicator of the culture as a whole, which becomes more differentiated and subject to more rapid change. But my interest here is on the impact of the *preindustrial* cultural heritage of given societies. Though religion is only a rough indicator of this heritage, I use it for want of more refined measures of the value systems prevailing at given times and places in the past.

Figure 7 diagrams a long-term process of economic and cultural change that led to the emergence of democracy in the West as one consequence. As this figure suggests, the relationship between economic and cultural change is one of complex reciprocal causality, with

Figure 7. Long-Term Economic Consequences of Rise of Protestant Ethic


TRADITIONAL VALUE SYSTEM
Emphasizes acceptance of inherited status in existing social order;
De-emphasizes economic aspirations; social mobility limited to narrow channels (e.g., priesthood Mandarin bureaucracy)
Obligation to share, give charity
STABLE SOCIAL AND ECONOMIC SYSTEM
Low economic growth
PROTESTANT REFORMATION
Traditional Value System
Stable Social and Economic System
PROTESTANT ETHIC
CAPITALISM
Rapid economic growth
PROSPERITY
POSTMATERIALIST VALUES
SLOWER ECONOMIC GROWTH

Note: With varying time lags, the impact of the industrialized nations serves as a functional equivalent to the Protestant Reformation for the rest of the world, though the value system that emerges is shaped by the traditional value system that precedes it.

cultural factors not only being influenced by economic change, but also influencing it. Weber argues that Calvinist Protestantism gradually evolved into a value system that viewed the accumulation of wealth for its own sake (and not as a means to survive or acquire luxuries) as a sign of divine grace and encouraged an ascetic self-control conducive to the accumulation of wealth. This led to an entrepreneurial spirit and an accumulation of capital that facilitated the Industrial Revolution in the eighteenth and nineteenth centuries, which in turn had immense consequences for global economic development in the twentieth century.

I suggest that the Protestant Reformation was only one case of a more general phenomenon: the breakdown of traditional cultural barriers to economic modernization. For as the top half of Figure 7 suggests, one feature common to traditional value systems is that they emerge in, and are adapted to, economies characterized by very little technological change and economic growth. In this situation, social mobility is a zero-sum game, heavily laden with conflict and threatening to the social system. In a society undergoing rapid industrialization and expansion, by contrast, social mobility may be widespread. But in traditional agrarian societies, social status is hereditary, except when an individual or group forcibly seizes the lands and social status of another. To preserve social peace, virtually all traditional cultures discourage upward social mobility and the accumulation of wealth. These cultures perform an integrating function by providing a rationale that legitimates the established social order and inculcating norms of sharing, charity, and other obligations that help to mitigate the harshness of a subsistence economy.

By their very nature the traditional value systems of agrarian society are adapted to maintaining a stable balance in unchanging societies. Accordingly, they tend to discourage social change in general and the accumulative entrepreneurial spirit in particular. One of the important functions of the Protestant Reformation was to break the grip of the medieval Christian world view on a significant portion of Europe. It did not accomplish this alone. The emergence of scientific inquiry had already begun to undermine the anthropocentric cosmos of the medieval Christian tradition. But it is difficult to avoid the conclusion that Weber's emphasis on the role of Protestantism captures an important part of reality. For prior to the Protestant Reformation, southern Europe was economically more advanced than northern Europe. During the three centuries after the Reformation, capitalism emerged, mainly among the Protestant regions of Europe and among the Protestant minorities of Catholic countries. Protestant Europe manifested a subsequent economic dynamism that was extraordinary, moving it far ahead of Catholic Europe. Shifting trade patterns, declining food production in southern Europe, and other variables played a role in this shift, but the evidence suggests that cultural factors were also important.

As capitalism led to industrialization and eventually to historically unprecedented levels of prosperity, emulation became more and more attractive, and increasing amounts of cross-cultural diffusion took place. But to a truly remarkable degree, throughout the early stages the Protestant cultural zone was markedly more receptive to industrialization and economic development than any other part of the world. The Industrial Revolution began in England, spreading rapidly to predominantly Protestant Scotland and Wales but leaving Catholic Ireland largely untouched except for the Protestant region around Belfast. Industrialization spread from England to nearby France but lagged there in comparison with its rapid implantation in more distant but more receptive areas such as the United States and Germany, both of which soon became far more industrialized than France. At the start of the twentieth century, the correlation between Protestantism and economic development was still remarkably strong. In Europe, the economically most dynamic nations were Great Britain, Germany, Sweden, Denmark, Norway, The Netherlands, and Switzerland, all of which were predominantly Protestant at that time. The only non-Protestant countries that had attained even roughly comparable levels of economic development were Belgium and France, both of which were geographically near the original core area from which the Industrial Revolution spread and in both of which Protestant minorities played a disproportionately important role in the process of economic development. In the New World, the United States and Canada had also emerged as developed industrial societies, while virtually all of Latin America remained almost totally unaffected by the Industrial Revolution. Even within Canada, the predominantly Catholic region developed much less rapidly than the rest of the country. Economic development seemed wedded to Protestantism.

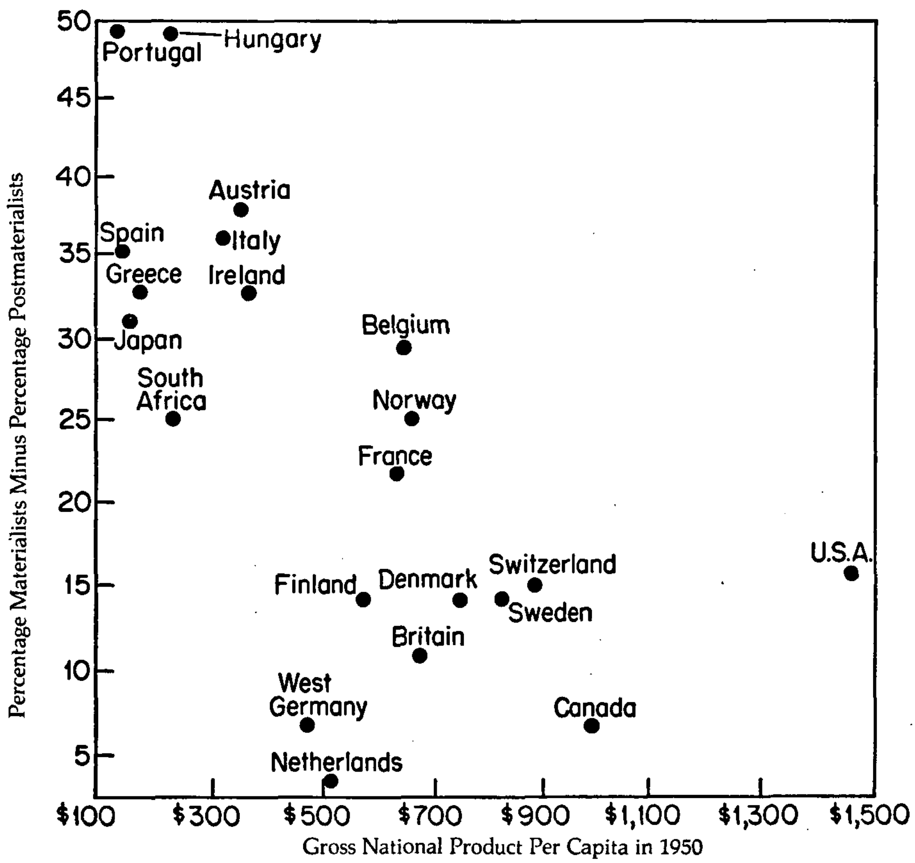
But culture is not a constant. It is a system through which a society adapts to its environment. Given a changing environment, in the long run culture is likely to change. One major change that took place was the secularization of Catholic (and other non-Protestant) cultures. In much of the world, the role of the merchant and the profit-making entrepreneur became less stigmatized. In some settings the entrepreneur even became the cultural hero, as the captain of industry had been in the United States of the late nineteenth century.

A contrasting process of cultural change began to take place in the more advanced industrial societies during the second half of the twentieth century. The lower half of Figure 7 diagrams this process. Precisely in those regions that had earlier been most strongly influenced by the Protestant ethic, the long-term consequences of economic development began to be felt, as generations emerged that had been raised in unprecedented prosperity and economic security and were characterized, increasingly, by the presence of postmaterialist values. The theoretical reasons underlying this process of value change—and a large body of supporting evidence—have been presented in detail elsewhere (Dalton 1977; Dalton, Flanagan, and Beck 1984; Deth 1984; Flanagan 1982; Inglehart 1971, 1977, 1981, 1985; Ike 1973; Kaase and Klingemann 1979; Lafferty and Knutsen 1985; Marsh 1975; Thomassen et al. 1983).

This thesis implies that as a result of the historically unprecedented prosperity and the absence of war that has prevailed in Western countries since 1945, younger birth cohorts place less emphasis on economic and physical security than do older groups, who have experienced a much greater degree of economic insecurity. Conversely, the younger birth cohorts tend to give a higher priority to nonmaterial needs, such as a sense of community and the quality of life. Cohort analysis carried out from 1970 through 1987 in six Western countries confirms the presence of substantial differences in the basic societal priorities of younger and older generations (Inglehart n.d.). Moreover, it demonstrates that as intergenerational population replacement has occurred, there has been a gradual but pervasive shift in the values of these publics from predominantly materialist priorities toward postmaterialist goals. One consequence of this shift has been a diminishing emphasis on economic growth in these societies together with increasing emphasis on environmental protection and preserving the quality of life—if necessary, even at the expense of economic growth. Postmaterialists place markedly less emphasis on economic growth than do those with materialist or mixed values. And they emphasize a high salary and job security less than working with people they like or doing interesting work (Inglehart 1977). Conversely, postmaterialists place more emphasis on protecting the environment and are far more likely to be active members of environmental organizations than are materialists. Finally, postmaterialists are economic underachievers; that is, *controlling* for the fact that they come from more prosperous families and receive better education, postmaterialists earn significantly lower incomes than those with materialist values (Inglehart n.d.). All this suggests that as societies become increasingly influenced by the growing postmaterialist minority, they will tend to give economic growth a lower priority. Figures 8 and 9 test this prediction at the societal level.

Evidence from a cross-national perspective converges with evidence from the individual level, pointing to a long-term cultural process of negative feedback linked with economic growth. On one hand, as Figure 8 demonstrates, the publics of relatively rich societies are least likely to emphasize materialist values and most likely to emphasize postmaterialist ones. Since one's values tend to reflect the conditions prevailing during one's preadult years, I allow a lag of about 30 years between the independent variable (level of development in 1950) and the dependent variable (mass value priorities in 1981–86). Since the median age in my adult sample is about 45, my economic indicator taps conditions when the median

Figure 8. Economic Development and the Decline of Materialist Values



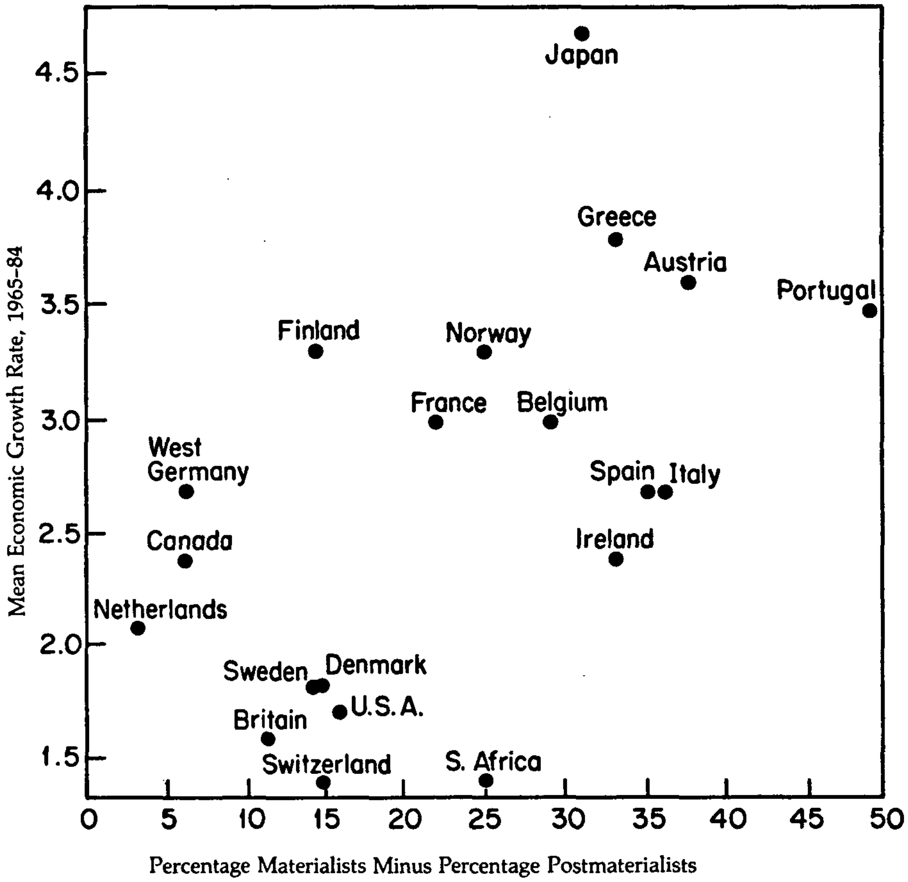
Sources: Value priorities data from World Values survey, 1981; Euro-Barometer surveys 19-25 (1982-1986); and Political Action study, 1974. Gross national product per capita calculated from *UN Statistical Yearbook, 1958* (New York: UN, 1959).

Note: $r = -.63$.

individual was about 15 years of age. I find a correlation of −.63 between a society's per capita GNP in 1950 and the proportion of materialists among that society's public in the 1980s. Not only does this result have the predicted polarity and significant strength, but it is stronger than the correlation obtained when I use GNP per capita in 1980 as the independent variable. The time lag assumed to exist between economic cause and cultural effect seems to reflect reality.

Figure 9 is a mirror image of Figure 8. The wealthier societies are least likely to produce materialist publics, but materialist publics seem to produce high economic growth rates. Or, to reverse labels, though wealthier societies are most likely to produce postmaterialists, after an appropriate time lag the more postmaterialist societies have the lowest growth rates. The long-term result is that high growth rates even-

Figure 9. Materialist Values and Economic Growth, 1965–84



Sources: Growth rates 1965–84 from *World Development Report, 1986* (Washington: World Bank, 1986). Growth rates 1870–1913 from Angus Maddison, *Economic Growth in Japan and the USSR* (London: Allen & Unwin, 1969).

Note: *r* = .51 (correlation between 1870–1913 growth rates and 1981–86 values = −.38, i.e., the countries that are most postmaterialist today had relatively high growth rates in 1870–1913).

tually lead to lower growth rates. Prosperity engenders a cultural shift toward postmaterialist values, which eventually leads to a less intense emphasis on economic growth.

Data from 21 societies reveal a consistent cultural-economic syndrome that was originally linked with the Protestant ethic: the wealthier nations and those with highly developed tertiary sectors are most likely to be long-established democracies, *and* the publics of these societies tend to show a "civic" political culture, have less materialist value priorities, and tend to be Protestant in religion (cf. Huntington 1984). But the Protestant ethic seems to be unraveling: for the linkage between Protestantism and economic

Table 2. Economic Growth Rates in Protestant vs. Catholic Countries and Japan, 1870–1984

Rank	1870–1913	1913–38	1949–65	1965–84
1	U.S.A. (P)	Japan (B)	Japan (B)	Japan (B)
2	Canada (P)	Norway (P)	W. Germany (P)	Norway (P)
3	Denmark (P)	Netherlands (P)	Italy (C)	France (C)
4	Sweden (P)	U.S.A. (P)	France (C)	Belgium (C)
5	Germany (P)	Switzerland (P)	Switzerland (P)	Italy (C)
6	Belgium (C)	Denmark (P)	Netherlands (P)	W. Germany (P)
7	Switzerland (P)	Sweden (P)	Canada (P)	Canada (P)
8	Japan (B)	Italy (C)	Denmark (P)	Netherlands (P)
9	Norway (P)	Canada (P)	Norway (P)	Denmark (P)
10	Gt. Britain (P)	Germany (P)	Sweden (P)	Sweden (P)
11	Netherlands (P)	Gt. Britain (P)	U.S.A. (P)	U.S.A. (P)
12	France (C)	France (C)	Belgium (C)	Gt. Britain (P)
13	Italy (C)	Belgium (C)	Gt. Britain (P)	Switzerland (P)
Mean economic growth rate in Protestant countries[a]	152	120	98	72

Sources: 1870–1965 standings calculated from data in Angus Maddison, *Economic Growth in Japan and the U.S.S.R.* (London: Allen & Unwin, 1969), pp. 148–49; 1965–84 standings from data in *World Development Report, 1986* (Washington: World Bank, 1986).

Note: (P) = countries in which a majority of the population was Protestant in 1900; (C) = countries having a Roman Catholic majority in 1900; (B) = Buddhist majority in 1900.

[a]Percentage of rates in Catholic countries.

achievement is a thing of the past. While the Protestant ethic syndrome was strongly correlated with high levels of economic growth in 1870–1913 (the earliest period for which we have data), this correlation weakens and then becomes strongly negative as we move into historically more recent periods.

Among those countries for which we have long-term historical data, those that had relatively high growth rates a century ago tend to have relatively low growth rates today. Table 2 illustrates this phenomenon. In 1870–1913, nearly all Protestant countries had growth rates that were higher than those of almost all Catholic countries. My table actually understates the extent to which this was true, because the few Catholic countries from which we have historical data are precisely the ones that were *most* developed in the nineteenth century. Protestant countries still had more dynamic economies in the interwar years. But in the past few decades, this situation has reversed itself; during 1965–84, the Catholic countries in our sample had *higher* growth rates than most of the Protestant ones. Within the United States, as recently as 1958, Catholics and Protestants manifested different values concerning various aspects of economic and family life (Lenski 1963). But these differences have been dwindling (Alwin 1986).

In part, this reflects the fact that it is easier for a poor country to attain a high growth rate than for a rich one. By importing technology that has already been proven in more developed countries, one can catch up rapidly. But in global perspectives it is clear that this is only part of the story. For plenty of poor societies are *not* showing rapid economic growth, while others (like those of East Asia) have been growing at an extraordinary pace. Clearly, some societies are more receptive to economic development than others. Conversely, some rich nations (like Japan) continue to develop relatively rapidly—even when they can no longer rely on imported technology but are increasingly developing their own—while others have become relatively stagnant.

High economic growth was once an almost uniquely Protestant phenomenon; today it has become global in scope and is *less* likely to be found in the Protestant societies than elsewhere. This does not mean that the civic culture that emerged in these societies will disappear. On the contrary, these countries are still becoming richer and on the whole, life satisfaction, political satisfaction, and trust have been gradually rising in recent years. The syndrome that linked Protestantism with wealth and democracy has become less distinctively Protestant because it is permeating other regions of the world.

It is doing so unevenly, however, with its spread being shaped by the cultural traditions of given societies. The Confucian system was virtually unique among traditional cultures in that it institutionalized a socially accepted and even honored channel for upward social mobility, based on nonviolent individual achievement rather than ascription. By passing a series of difficult academic examinations that were open to any promising young male, one could attain power, status, and wealth as a government official. Consequently, in the sixteenth century a social scientist from Mars might have ranked East Asia, with its Confucian tradition, as the region of the world that was culturally most conducive to economic development. Though narrowly circumscribed, social mobility through individual achievement was accepted to a degree virtually unknown elsewhere. Education, rather than armed force, was the principal means to rise in society. And a secular orientation was relatively conducive to technology and worldly achievement.

I suspect that the Confucian cultural tradition, its traditional rigidity having been shattered by the impact of the West, is an important element underlying the current economic dynamism of certain portions of Asia. During the period from 1965 to 1984, 5 of the 10 fastest-growing nations in the world were countries shaped by the Confucian and Buddhist traditions: Singapore, South Korea, Hong Kong, Taiwan, and Japan. China ranked thirteenth. Moreover, three more of the top 20 countries had significant Chinese minorities that in each case played disproportionately important economic roles: Malaysia, Thailand, and Indonesia. Finally, immigrants of East Asian origin have shown disproportionately high rates of economic achievement throughout Southeast Asia and in the United States, Canada, and Western Europe. It is difficult to avoid the conclusion that the Confucian cultural tradition is conducive to economic achievement today. It would be unrealistic to view these traits as indelible, however. My broader thesis suggests that the intense emphasis on economic achievement now found among peoples shaped by the Confucian tradition could emerge only when the static orientation of traditional society was broken and is likely to gradually erode when future generations have been raised in high levels of economic security. For the present, however, it may be a key factor in the world economy.

Conclusion

Both social analysis and social policy would be much simpler if people from different societies were interchangeable robots. But a large body of evidence indicates that they are not. The peoples of given societies tend to be characterized by reasonably durable cultural attributes that sometimes have major political and economic consequences. If this is true, then effective social policy will be better served by learning about these differences and about how they vary cross-culturally and over time than by pretending that they don't exist.

Rational choice models constitute one of the most promising tools now available for political analysis. As currently applied, they are effective in analyzing short-term fluctuations within a given system, taking cultural and institutional factors as constant. But these factors are *not* constant, either cross-nationally or over time. And current models cannot deal with long-term changes in the basic goals and nature of a system. One of the central debates in the field of political economy seems to reflect this fact. When it was found that political support responded to fluctuations in the economy, it was taken for granted that this reflected the workings of economic self-interest among the electorate. Subsequent research has made this interpretation increasingly doubtful. The linkage between economics and politics seems largely shaped by sociotropic concerns. The classic model of economically determined behavior has a strong grip on the minds of social analysts, probably because, throughout most of the history of industrial society, it provided a fairly accurate description of human behavior. In recent decades, the rising role of postmaterialist concerns may have helped make sociotropic concerns increasingly important, particularly among the politically more aware segments of the electorate.

Political economy research has demonstrated convincingly that short-term economic changes have significant political consequences. But the long-term consequences of economic change have barely begun to be analyzed in comparable fashion, though they may be at least equally significant. Evidence presented here indicates that the emergence and viability of mass-based democracy is closely related to economic development *and* that the outcome is contingent on specific cultural changes. Though mass democracy is almost impossible without a certain amount of economic development, economic development by itself does not produce democracy. Unless specific changes occur in culture and social structure, the result may not be democracy but a variety of alternatives ranging from the Libyan to the Soviet. A large body of cross-national survey evidence indicates that enduring cultural differences exist. Though these differences may be related to the economic level of a given nation, they are relatively independent of short-term economic changes. These cultural factors have an important bearing on the durability of democracy, which seems to result from a complex interplay of economic, cultural, and institutional factors. To neglect any of these components may compromise its survival.

Finally, it appears that economic development itself is influenced by cultural variables. In my brief analysis of this, I have utilized one indicator of materialist and postmaterialist values that is available from only the past two decades and another indicator—the dominant religious or philosophic tradition of a given society—that is a greatly oversimplified indicator of prevailing worldviews at a given time and place but goes back over centuries. Clearly, this analysis cannot be regarded as conclusive. But the available evidence tends to confirm Weber's insight that culture is not just a consequence of economics but can shape the basic nature of economic and political life.

Note

1. These findings, incidentally, bear on a question that was recently considered grounds for denying Samuel Huntington membership in the National Academy of Sciences. His opponents charged that it was absurd to argue that relatively high or low levels of satisfaction can be attributed to given nations; but in a very significant sense, it appears that they *can*.

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CONDORCET'S THEORY OF VOTING

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Condorcet's criterion states that an alternative that defeats every other by a simple majority is the socially optimal choice. Condorcet argued that if the object of voting is to determine the "best" decision for society but voters sometimes make mistakes in their judgments, then the majority alternative (if it exists) is statistically most likely to be the best choice. Strictly speaking, this claim is not true; in some situations Borda's rule gives a sharper estimate of the best alternative. Nevertheless, Condorcet did propose a novel and statistically correct rule for finding the most likely ranking of the alternatives. This procedure, which is sometimes known as "Kemeny's rule," is the unique social welfare function that satisfies a variant of independence of irrelevant alternatives together with several other standard properties.

> [Our aim is] to inquire by mere reasoning, what degree of confidence the judgment of assemblies deserves, whether large or small, subject to a high or low plurality, split into several different bodies or gathered into one only, composed of men more or less wise.
>
> —Condorcet

A central problem in democratic theory is to justify the principle of majority rule. Why should the opinion of a majority of citizens or of elected representatives bind the rest of society? One of the earliest and most celebrated answers to this question was attempted by Rousseau in *The Social Contract* (1762). The opinion of the majority is legitimate, said Rousseau, because it expresses the "general will." "When a law is proposed in the people's assembly, what is asked of them is not precisely whether they approve of the proposition or reject it, but whether it is in conformity with the general will . . . each by giving his vote gives his opinion on this question, and the counting of votes yields a declaration of the general will. When, therefore, the opinion contrary to my own prevails, this proves only that I have made a mistake, and that what I believed to be the general will was not so" (1962, 153).

This ambiguous and slightly disquieting idea was given a more satisfactory expression some twenty years later by the mathematician and social philosopher Marie Jean Antoine Nicolas Caritat, Marquis de Condorcet.[1] In his remarkable work, *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix* (1785), Condorcet proposed the following argument for decision by majority: Enlightened voters honestly attempt to judge what decision will best serve society. They may occasionally judge wrongly. But assuming that they are more often right than wrong, the majority opinion will very likely be "correct." This is just a property of large numbers and is intuitively obvious when there are only two possible decisions. Condorcet rigorously demonstrated this proposition using the newly developed calculus of probabilities. He then proceeded to show how the idea could be elaborated into a whole series of results about the

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reliability of decisions of a voting body, depending on its size, the competence of its members, and the number of alternatives under consideration. These "jury theorems" have recently attracted considerable attention (Grofman and Owen 1986; Grofman, Owen, and Feld 1983; Urken and Traflet 1984).[2]

It has not been widely recognized, however, that Condorcet showed how the same arguments could be extended to the case of more than two decisions. He was, of course, the first to realize the complications that arise in this case, namely, the possibility of cyclic majorities. But he went on to suggest—or at least came very close to suggesting—a definite rule for ranking any number of alternatives even when cyclic majorities are involved. Admittedly, Condorcet's argument is difficult to follow and full of inconsistencies. Moreover, the rule that he proposed is at variance with the algorithm that he proposed for computing it. Nevertheless, his stated goal is clear: to find the social decision that is most likely to be "correct."

First I summarize Condorcet's proposal and dispose of the claims made by some commentators that Condorcet gave no solution for the general case. I then show that Condorcet's method can be interpreted as a statistical procedure for estimating the ranking of the candidates that is most likely to be correct. If only one candidate is to be selected, however, then an earlier method proposed by Condorcet's rival Borda often gives better results. In the final section I show that Condorcet's decision rule can also be justified in modern social choice terms. It is the unique ranking procedure that satisfies a variant of independence of irrelevant alternatives together with several other standard conditions in social choice theory.

Condorcet's Model

For Condorcet, as for Rousseau, the object of voting is not merely to balance subjective opinions; rather, it is a collective quest for truth. In any field of candidates (or agenda of alternatives) there is some truly best candidate, a truly second-best candidate, and so forth. What is meant here by truly best? Ultimately it is an undefined term, a notion that is postulated in order to make the theory work—like the ether in classical physics. To be sure, in some situations the notion of "best" decision is quite realistic. An example is trial by jury, where the group decision problem is to determine guilt or innocence. Another is the pooling of expert opinions about an observable event, like the state of the economy or tomorrow's weather.[3]

For the present let us grant Condorcet's hypothesis that the notion of a "correct" decision also applies to political decision making. Thus, voters evaluate candidates for political office in terms of their ability to lead. Legislators consider bills in terms of their potential contribution to the public welfare. Granted that voters are at least sometimes mindful of the public interest and vote accordingly, Condorcet's argument proceeds along the following lines. People differ in their opinions because they are imperfect judges of which decision really is best. If on balance each voter is more often right than wrong, however, then the majority view is *very likely* to identify the decision that *is* objectively best. In other words, the majority opinion gives the best estimate, in a statistical sense, of which candidate is most likely to be best. One of Condorcet's major contributions was to show how this idea can be generalized to the case of many candidates.

Consider a list of candidates (or decisions) that are voted upon pairwise. Assume that each voter, when faced with a comparison of the form, "Is *a* better than *b* or *b* better than *a*?" chooses correctly with some fixed probability greater than 1/2. Each voter judges every pair of candidates and the results are tallied in a voting matrix. Table 1 shows a voting matrix with 13 voters and three candidates. Each entry in the matrix gives the total number of votes obtained by the row candidate over the column candidate. For example, the comparison *a* versus *b* yields eight votes in favor of *a*, five in favor of *b*. The comparison *b* versus *c* yields eleven votes for *b* and two for *c*. The comparison *a* versus *c* yields six for *a* and seven for *c*. Notice that this vote entails the cyclical majority $a > b$, $b > c$, $c > a$.

Given a series of pairwise votes as in Table 1, the problem posed by Condorcet is to find the ranking of all the candidates that is *most likely* to be correct, that is, the ranking that would have produced the observed votes with highest probability. At first blush it might appear that this problem is insoluble unless the competency levels of the voters are known. This difficulty can be circumvented if one makes the simplifying assumption (which Condorcet did) that the voters all have the same competence. In this case the answer turns out to be independent of the level of competence, as I shall show in the next section. Here we shall content ourselves with Condorcet's statement of the solution.

First some terminology is required. In Condorcet's lexicon, an "opinion" is a series of pairwise comparisons on the alternatives. Each pairwise comparison is called a "proposition" and written $a > b$, etc. An opinion is said to be "impossible," "contradictory," or "absurd" if some of the propositions composing it form a cycle, such as $a > b$, $b > c$, $c > a$. Normally, each individual voter is able to rank all of the candidates in a consistent order. This is not necessarily true of the aggregate opinion, however, as Table 1 demonstrates.[4] To break such cyclic majorities, Condorcet proposed the following method:[5]

Table 1. Voting Matrix with Three Alternatives and 11 Voters

	a	*b*	*c*
a	—	8	6
b	5	—	11
c	7	2	—

> 1. All possible opinions that do not imply a contradiction reduce to an indication of the order of merit that one judges to exist among the candidates . . . . therefore for n candidates one would have $n(n-1)\ldots 2$ possible opinions.
>
> 2. Each voter having thus given his or her opinion by indicating the candidates' order of worth, if one compares them two by two, one will have in each opinion $n(n-1)/2$ propositions to consider separately. Taking the number of times that each is contained in the opinion of one of the q voters, one will have the number of voices who are for each proposition.
>
> 3. One forms an opinion from those $n(n-1)/2$ propositions that agree with the most voices. If this opinion is among the $n(n-1)\ldots 2$ possible opinions, one regards as elected the subject to whom this opinion accords the preference. If this opinion is among the $2^{n(n-1)/2} - n(n-1)\ldots 2$ impossible opinions, then one successively deletes from that impossible opinion the propositions that have the least plurality, and one adopts the opinion from those that remain. (1785, 125–26)

Let us apply this rule to the situation in Table 1. First we are to choose the three propositions having a majority, namely $b > c$ with eleven votes, $a > b$ with eight votes, and $c > a$ with seven votes. Since these three propositions form a cycle, however, we delete the proposition with smallest plurality, namely $c > a$. This leaves the combination $b > c$ and $a > b$ (and implicitly $a > c$), which implies the ranking $a > b > c$.

The rule is clear enough in the case of three alternatives. However, in the general case some ambiguities arise. The difficulty lies in the phrase "successively deletes" in step 3. Taken literally, it means the following: If there are cycles in the propositions selected in step 2, delete first the proposition having the least number of votes in its favor. (This proposition

Table 2. A Voting Matrix with 25 Voters and Four Alternatives

	a	*b*	*c*	*d*
a	—	12	15	17
b	13	—	16	11
c	10	9	—	18
d	8	14	7	—

might or might not be contained in a cycle.) If any cycles remain, delete next the proposition with the next lowest number of votes in its favor, and so forth. Continue until all cycles are eliminated.

There are several reasons why this is probably not what Condorcet meant to say. In the first place, the procedure may yield an ambiguous ordering of the candidates. Consider Table 2, with twenty-five voters and four candidates.

In step 2 of Condorcet's algorithm one would select the six propositions having greatest majorities. In descending size of majority, these are $c > d$, $a > d$, $b > c$, $a > c$, $d > b$, $b > a$. According to a literal reading of step 3, one would first delete the proposition $b > a$, as it has the smallest majority in its favor. But this does not result in an "opinion" because one cycle still remains: $b > c$, $c > d$, $d > b$. Therefore one would delete the proposition $d > b$, as it has the next-smallest majority in its favor. All cycles are now eliminated. But there is a difficulty: in the resulting partial order both *a* and *b* are undominated. Either one of them could be interpreted as the top-ranked candidate, so the outcome is indeterminate.

It seems more likely that Condorcet meant to *reverse*, rather than simply *delete*, the weakest propositions. According to this interpretation one would first reverse the proposition with the least majority (thus $b > a$ becomes $a > b$), then reverse the proposition with the next-least majority (thus $d > b$ becomes $b > d$) and so forth until no cycles remain. In the above example the resulting opinion is *abcd*. Unfortunately, this answer does not square with the rest of Condorcet's argument. His stated intention is to choose "the most probable combination of opinions," that is, the set of propositions that contains no cycles and is supported by the largest number of pairwise votes. The total number of votes supporting the ranking *abcd* is 89. (That is, the sum of the votes for *a* over *b*, *a* over *c*, *a* over *d*, *b* over *c*, *b* over *d*, and *c* over *d* equals 89.) But this is not a maximum. The ranking *bacd* is supported by a total of 90 pairwise votes. This ranking is obtained by reversing exactly one majority proposition, $d > b$. Note that this is not the proposition with smallest majority.

At this point we might be tempted to throw up our hands and declare, as previous commentators have, that "the general rules for the case of any number of candidates as given by Condorcet are stated so briefly as to be hardly intelligible . . . and as no examples are given it is quite hopeless to find out what Condorcet meant" (E. J. Nanson as quoted in Black 1958, 175). Even the influential mathematician Isaac Todhunter found Condorcet's argument completely exasperating: "The obscurity and self-contradiction are without any parallel, so far as our experience of mathematical works extends . . . no amount of examples can convey an adequate impression of the evils" (Todhunter 1949, 352).

In fact, Condorcet's overall intention is quite clear. It is to find *the most probable combination of opinions*. In describing his method he seems to have used the term "successively deletes" rather loosely as a heuristic for computing the solution.[6] (He may also have believed, mistakenly, that successive deletion always does give the most probable combination.) But there appears to be little doubt about his objective. Hence I propose to amend Condorcet's statement of the rule as follows:

> 3′. One forms an opinion from those $n(n-1)/2$ propositions that agree with the most voices. If this opinion is among the $n(n-1)\ldots2$ possible opinions, one regards as elected the subject to whom this opinion accords the preference. If this opinion is among the $2^{n(n-1)/2} - n(n-1)\ldots2$ impossible opinions, then one *reverses in* that impossible opinion the *set of* propositions that have the least *combined* plurality and one adopts the opinion from those that remain.

It seems reasonably likely that this is what Condorcet meant to say. In any case, it is the only interpretation that is consistent with his goal of finding the ranking that is most likely to be correct.

Condorcet's Method As a Statistical Hypothesis Test

From a historical standpoint, Condorcet's theory is particularly interesting because it represents one of the earliest applications of what today would be called "statistical hypothesis testing." Based on a sample of observations, one wishes to infer which of several underlying but unobservable states of nature is most likely to be true. This approach is known as *maximum likelihood estimation.* All that is required is a model of how the observed quantities depend probabilistically on the unobservable state of nature. In Condorcet's scheme of things a state of nature corresponds to the true or correct ordering of the candidates. This datum is essentially unobservable. What can be observed are the votes of the representatives who are attempting to read the true state of nature.

Condorcet assumed, first, that in any pairwise comparison each voter will choose the better candidate with some fixed probability p, where $1/2 < p < 1$ and p is the same for all voters.[7] Second, every voter's judgment on every pair of candidates is independent of his or her judgment on every other pair. Finally, each voter's judgment is independent of the other voters' judgments (a significant assumption).

Consider again the voting matrix in Table 1. Suppose that the true ranking is *abc*—*a* first, *b* second, *c* third. A vote for *a* over *b* will occur with probability $1 - p$, whereas a vote for *b* over *a* will occur with the lesser probability $1 - p$. The combined probability of observing the 39 pairwise votes in Table 1 is therefore

$$L(abc) = \frac{13!}{8!5!}[p^8(1-p)^5]$$

$$\frac{13!}{6!7!}[p^6(1-p)^7]\ \frac{13!}{11!2!}[p^{11}(1-p)^2]$$

$$= \frac{(13!)^3}{8!5!6!7!11!2!}[p^{25}(1-p)^{14}].$$

Similarly, the probability of observing the results in Table 1 if the true ranking is *acb* would be

$$L(abc) = \frac{13!}{8!5!}[p^8(1-p)^5]$$

$$\frac{13!}{6!7!}[p^6(1-p)^7]\ \frac{13!}{11!2!}[p^2(1-p)^{11}]$$

$$= \frac{(13!)^3}{8!5!6!7!11!2!}[p^{16}(1-p)^{23}].$$

The coefficient is the same for all six rankings, so their relative likelihoods are as in Table 3.

Table 3. Likelihoods of Six Rankings Based on Table 1 Data

Ranking	Likelihood
abc	$p^{25}(1-p)^{14}$
abc	$p^{16}(1-p)^{23}$
bac	$p^{22}(1-p)^{17}$
bca	$p^{23}(1-p)^{16}$
cab	$p^{17}(1-p)^{22}$
cba	$p^{14}(1-p)^{25}$

For each ranking the exponent of p is the sum of all pairwise votes that agree with the ranking, and the exponent of $(1 - p)$ is the sum of all pairwise votes that disagree with it. Assuming that $p > 1/2$, which seems to be a credibly optimistic view of human judgment, it follows that the most likely ranking is the one with the highest exponent of p. In the above example the answer is *abc*.

In general, suppose that there are n voters and m candidates or alternatives $a_1, \ldots, a_m$. Let n_{ij} be the total number of votes that a_i receives over a_j. Assume that every voter registers an opinion on every pair of candidates, so that $n_{ij} + n_{ji} = n$ for all $i \neq j$. Let R be a complete ranking of the candidates, where $a_i R\, a_j$ means that a_i is preferred to a_j. The argument outlined above is easily generalized to show that a ranking R has maximum likelihood if and only if it maximizes Σn_{ij}, the sum over all pairs ij such that $a_i R a_j$. That is, the maximum likelihood ranking is the one that agrees with the maximum number of pairwise votes, summed over all pairs of candidates. This is equivalent to the statement of Condorcet's rule given in 3′. Note that it does not depend on the value of the success rate p as long as all voters have the same success rate, which is larger than one half. This rule is very natural and, not surprisingly, has been rediscovered by other writers. In particular it was proposed in a different guise by John Kemeny (1959; see also Kemeny and Snell 1960) and is sometimes known in the literature as "Kemeny's rule" (Young and Levenglick 1978).

Condorcet versus Borda

Another pioneer in the theory of elections was the Chevalier Jean Charles de Borda. Like Condorcet, Borda was a member of the French Academy of Sciences and an important figure in official scientific circles. Unlike Condorcet, he was an applied scientist with a practical bent. He was noted for his work in hydraulics, mechanics, optics, and especially for the design of advanced navigational instruments. He also captained maritime vessels. Because of his expertise in measurement, he served on the committee for determining the standard weights and measures in the new metric system (Mascart 1919).

In 1770, some 15 years prior to the publication of Condorcet's treatise on elections, Borda read a paper to the academy on the design of voting procedures (see Borda 1784). Exactly what prompted Borda to sail in these uncharted waters is unclear. Possibly he was concerned with reforming the election procedures in the academy itself. He began by pointing out the defects with the customary "first-past-the-post" system in which the candidate with the largest plurality is elected. When there are many candidates, this procedure can easily elect someone who is endorsed by only a small minority of the electorate. Instead, Borda suggested, the election procedure should be based on each voter's entire rank ordering of the candidates. The correct candidate to choose is the one that on average stands highest in the voters' lists.

Operationally, the method works as follows: Each voter submits a list in which all of the candidates are rank-ordered. In each list a candidate who is ranked last receives a score of zero, a candidate who is ranked next-to-last receives a score of one, a candidate who is ranked next above that receives a score of two, and so forth. Thus the score of a candidate in any particular list is equal to the number of candidates ranked lower on that list. The scores of the candidates are summed over all voter lists and the candidates with highest total score is elected.

If Borda's objective was practical reform, he appears to have been successful, for something like his method was subsequently adopted for the election of new members to the academy.[8]

Condorcet was certainly aware of Borda's work. Indeed he introduced the method in the *Essai* with the following remark: "As the celebrated Geometer to whom one owes this method has published nothing on this subject, I have taken the liberty of citing it here" (1785, clxxix).[9] Condorcet repeatedly refers to Borda, not by name, but as "the celebrated Geometer." The tone is ironical. From his private correspondence it is clear that Condorcet held Borda's work in low esteem and considered him a personal enemy within the academy. For example, in a letter to Turgot (1775), he gave this assessment: "[M. Malesherbes] makes a great case for Borda, not because of his memoirs, some of which suggest talent (although nothing will ever come of them, and no one has ever spoken of them or ever will) but because he is what one calls a good academician, that is to say because he speaks in meetings of the Academy and asks for nothing better than to waste his time doing prospectuses, examining machines, etc., and above all because, feeling eclipsed by other geometers, he, like d'Arcy, has abandoned geometry for petty physics" (quoted in Henry 1883). The contempt with which Condorcet viewed Borda's scientific contributions may help to clarify an abrupt shift in Condorcet's argument. As we have already seen, Condorcet's initial objective was to design a method for estimating the "true" ranking of the candidates. But he also recognized that the problem is often one of determining which single candidate is most likely to be best (1785, lxi–lxv, 122–23). At first glance the solution would appear to be obvious: first rank all of the candidates and then choose the one that is top-ranked. But this is not necessarily the most convincing answer.

Consider the following example of Condorcet (1785, lxiii) with 60 voters and three candidates. Here *c* has a simple majority over both *a* and *b* and *b* has a simple majority over *a*. So there are no cycles, and the ranking by Condorcet's rule is *cba*. That is, *cba* is the ranking most likely to be correct. But is *c* the candidate that is most likely to be the best (i.e., top-ranked in the true ranking)?

Table 4. A Voting Matrix with 60 Voters and Three Candidates

	a	*b*	*c*
a	—	23	29
b	37	—	29
c	31	31	—

Not necessarily. Candidate *c* will be best if two propositions hold: namely, if *c* is better than *a* ($c > a$) and *c* is better than *b* ($c > b$). We are therefore interested in the joint probability of these two statements being true. If $c > a$, then the probability of observing the voting pattern in Table 4 (31 votes for *c* versus 29 for *a*) is $(60!/31!29!)p^{31}(1 - p)^{29}$. If the contrary holds ($a > c$), then the probability of observing 31 votes for *c* versus 29 for *a* is $(60!/31!29)p^{29}(1 - p)^{31}$. Assume that $a > c$ and $c > a$ are a priori equally likely. Then the probability of observing the vote 31 for *c*, 29 for *a* is

$$Pr(\text{vote}) = (60!/31!29!)\,(p^{31}(1 - p)^{29} + p^{29}(1 - p)^{31})/2.$$

By Bayes' rule[10] it follows that the posterior probability of $c > a$, given the observed vote, is

$$Pr(c > a|\text{vote}) = \frac{Pr(\text{vote}|c > a)Pr(c > a)}{Pr(\text{vote})},$$

that is,

$$Pr(c > a|\text{vote}) = \frac{p^{31}(1 - p)^{29}}{p^{31}(1 - p)^{29} + p^{29}(1 - p)^{31}}.$$

Similarly,

$$Pr(c > b|\text{vote}) = \frac{p^{31}(1 - p)^{29}}{p^{31}(1 - p)^{29} + p^{29}(1 - p)^{31}}.$$

Therefore the posterior probability that c is best is

$$Pr(c|\text{vote}) =$$

$$Pr(c > a|\text{vote})Pr(c > b|\text{vote}) = \frac{[p^{31}(1-p)^{29}]^2}{[p^{31}(1-p)^{29} + p^{29}(1-p)^{31}]^2}.$$

The posterior probability that b is best is

$$Pr(b|\text{vote}) =$$

$$\frac{p^{37}(1-p)^{23}p^{29}(1-p)^{31}}{[p^{37}(1-p)^{23} + p^{23}(1-p)^{37}] \times [p^{29}(1-p)^{31} + p^{31}(1-p)^{29}]}.$$

And the posterior probability that a is best is

$$Pr(a|\text{vote}) =$$

$$\frac{p^{23}(1-p)^{37}p^{29}(1-p)^{31}}{[p^{23}(1-p)^{37} + p^{37}(1-p)^{23}] \times} \qquad + p^{31}(1-p)^{29}]\}.$$

Which of a, b, or c has the highest posterior probability? The answer depends on the assumed value of the competence level p. If p is close to one, then the controlling factor is the size of the exponent of $1 - p$. The most probable candidate will be the one whose *weakest* comparison is as strong as possible.[11] In this case the answer would be c, because the lowest number of votes that c receives in any pairwise contest is 31, while a's weakest showing is 23 and b's is 29.

Suppose instead that p is close to 1/2, that is, $p = 1/2 + \epsilon$ for some small $\epsilon > 0$. In this case the relative probabilities depend essentially only on the numerators of the above three expressions. Substituting in $p = 1/2 + \epsilon$ we find that the relative probabilities of a, b, and c can be approximated as follows:

a: $(1/2 + \epsilon)^{23}(1/2 - \epsilon)^{37}(1/2 + \epsilon)^{29}(1/2 - \epsilon)^{31}$
b: $(1/2 + \epsilon)^{37}(1/2 - \epsilon)^{23}(1/2 + \epsilon)^{29}(1/2 - \epsilon)^{31}$
c: $(1/2 + \epsilon)^{31}(1/2 - \epsilon(^{29}(1/2 + \epsilon)^{31}(1/2 - \epsilon)^{29}$

If we expand these expressions using the binomial formula and drop all powers of ϵ higher than the first, then the relative probabilities of the three candidates are approximately as follows:

a: $(1/2)^{120} + (1/2)^{119}(23 - 37 + 29 - 31)\epsilon$
b: $(1/2)^{120} + (1/2)^{119}(37 - 23 + 29 - 31)\epsilon$
c: $(1/2)^{120} + (1/2)^{119}(31 - 29 + 31 - 29)\epsilon$.

In each case the coefficient of ϵ is the sum of all pairwise votes for the candidate minus the sum of all pairwise votes against the candidate. Since the total number of votes (for and against) is the same for all candidates, the coefficient of ϵ will be maximized for that candidate that receives the most pairwise votes over all others. In this case the answer is b.

This argument is quite general. For any number of voters and any number of alternatives, if p is sufficiently close to 1/2, then the candidate with the highest probability of being best is the one that receives the most pairwise votes. This is precisely the Borda candidate. The reason is that the Borda score of a candidate in each individual voter's list is equal to the number of candidates ranked below it, that is, the number that it beats. So a candidate's total Borda score is its row sum in the voting matrix.[12]

Borda's method is therefore one solution to Condorcet's problem. In particular, a candidate that beats every other by simple majority does *not* necessarily have the strongest claim to being the best candidate; the Borda winner may be stronger.[13] Condorcet apparently realized this implication of his argument. Understandably, he would be loath to draw much attention to it, given his feelings toward Borda. In addition, Condorcet may have become disillusioned with his approach because of its failure to provide a simple and definitive solution independently of the competence level p.

For whatever reason, Condorcet abruptly changed course at this point in the *Essai* (1785, lxiii–lxv). He abandoned the statistical framework that he had so painstakingly built up and fell back on a more "straightforward" line of reasoning. In doing so he opened up a whole new approach that has had enormous influence on the modern theory of social choice. To illustrate Condorcet's newfound argument, consider the example in Table 4. If a candidate, such as *c*, obtains a simple majority over every other candidate then, Condorcet argued, that candidate is the only reasonable choice. Indeed, candidate *a* has no reasonable claim to being best, therefore *a* should be set aside. So the contest is between *b* and *c*. But *c* defeats *b*, so the choice must come down on the side of *c*.

More generally, Condorcet proposed that whenever a candidate obtains a simple majority over every other candidate, then that candidate is presumptively the "best." This decision rule is now known as "Condorcet's criterion," and such a candidate (if it exists) is a "Condorcet winner" or a "majority candidate" (Fishburn 1973).

In advancing this new approach, Condorcet was not abandoning his basic premise that the object of voting is to make the "correct" choice. He merely shifted his ground and argued for adopting a simple estimate of the best choice rather than a complex one. Furthermore, this "new" approach is entirely consistent with Condorcet's earlier solution of the ranking problem. Indeed, if a majority candidate exists, then it must be ranked first in a Condorcet ranking. The reason is simple. Suppose that *x* is a Condorcet candidate but that this candidate is not ranked on top in the Condorcet ranking. Then it must be ranked just below some other candidate *y*. By assumption, *x* defeats *y* by a simple majority. Therefore, if we switch only the order of *x* and *y* in the ranking, then we obtain a new ranking that is supported by more pairwise votes. But this contradicts the definition of the Condorcet ranking as the one supported by the *maximum* number of pairwise votes. Thus Condorcet's choice rule is consistent with his ranking rule whenever a majority candidate exists.

While Condorcet's solution is attractive, Borda's rule still has advantages that cannot be dismissed easily. First, it is considerably simpler to compute than Condorcet's ranking rule. Second, if one accepts Condorcet's basic premises, then the Borda winner is in fact a better estimate of the best candidate provided that p is close to 1/2. Third, if p is *not* close to 1/2, then it is still *very likely* that the Borda winner is the best candidate, even though strictly speaking it may not be the optimum estimate of the best candidate.[14] If there are a large number of voters and p is not very close to 1/2, then the probability is very high that the truly best candidate will be selected by *any* reasonable choice rule (i.e., with high probability it will be the majority winner and the Borda winner at the same time). The critical case is precisely when p is near 1/2, and then Borda's rule is definitely optimal. Thus we must either jettison the whole idea of selecting the "best" candidate with high probability or admit that Borda's rule *is* a good way of estimating which candidate that is.

On the other hand, when the problem is to rank a set of alternatives, Condorcet's rule is undoubtedly better than Borda's. Furthermore, even if one does not accept the specific probability model on which this conclusion is based, Condorcet's rule has an important stability property that Borda's rule lacks, as I shall now show.

Local Stability and Nonmanipulability

One of the most important concepts in the theory of social choice is independence: a social decision procedure should depend only on the voters' preferences for the "relevant" alternatives, not on their preferences for alternatives that are infeasible or outside the domain of discourse. Independence has been given several distinct formulations (Arrow 1963;

Table 5. A Voting Matrix on Six Alternatives and 100 Voters

	a	*b*	*c*	*d*	*e*	*f*	Borda Scores
a	—	51	54	58	60	62	285
b	49	—	68	56	51	58	283
c	46	32	—	70	66	75	289
d	42	44	30	—	41	64	221
e	40	48	34	59	—	34	215
f	38	42	25	36	66	—	207

Nash 1950). The one that I shall adopt here is closest in spirit to Nash's version: if the set of alternatives shrinks, then the reduced set of alternatives should be ranked in the same way that they were within the larger ranking. There are at least two reasons why independence is desirable. First, it says that decisions cannot be manipulated by introducing extraneous alternatives. Second, it allows sensible decisions to be made without requiring an evaluation of all possible decisions. In practice, society cannot consider all possible choices in a situation. It cannot, or at least usually does not, even consider all of the best choices. In choosing a candidate for public office, for example, it is impractical to consider all eligible citizens. In considering a piece of legislation, it is impossible to hold votes on all possible amendments. Independence assures that a decision made from a limited agenda of alternatives is valid *relative to* that agenda. Enlarging the agenda may introduce better possibilities, but it should not cause us to revise the relative ranking of the old possibilities.

Unfortunately, it is impossible to design any reasonable social decision rule with this property. I claim, however, that independence can be satisfied if we restrict ourselves to agendas that are sufficiently "connected." By a connected agenda I mean, roughly speaking, a subset of closely related alternatives rather than an arbitrary collection of unrelated or extreme ones. Consider the following example: A legislature of 100 members is considering three alternative decisions *a, b,* and *c* (see Table 5). Other alternatives might in theory be considered, such as *d, e,* and *f,* but they are weaker than *a, b,* and *c* in the sense that any one of them would be defeated by any one of *a, b,* or *c.*

The question is whether the exclusion of the nominally "inferior" alternatives *d, e, f* affect the choice between *a, b,* and *c.* Put differently, can an inferior alternative be introduced strategically in order to manipulate the choice between the "superior" alternatives? This is surely a situation that we would like to avoid. Unfortunately Borda's rule suffers from this defect. To see this, observe that the total Borda score of each alternative is simply the sum of the row entries corresponding to that alternative (e.g., the right-hand column of Table 5). These scores imply the ranking *cabdef.* Now consider just the three alternatives *a, b,* and *c.* The corresponding voting matrix is the one enclosed by dashed lines. The Borda scores for this three-alternative situation are 117 for *b,* 105 for *a,* and 78 for *c.* Hence when just the top three alternatives are considered, they would be ordered *bac,* which is the *reverse* of how the electorate would have ordered them in the context of all alternatives. Introducing the inferior alternatives changes the outcome from *b* to *c.*

I claim that Condorcet's rule is immune

to this type of instability. To compute the Condorcet solution observe first that *a* has a simple majority over every other alternative, hence (as was demonstrated in the previous section) Condorcet's rule must rank *a* first. Since each of the alternatives *a*, *b*, and *c* defeats each of the alternatives *d*, *e*, and *f* by a majority, it also follows that the former three alternatives must be ranked above the latter three. For if this were not the case, then some alternative would be ranked immediately below another alternative that it defeats, which is impossible in a Condorcet ranking. To maximize agreement with the pairwise voting data, *a* must be ranked first, *b* second, and *c* third. Positions four, five, and six in the ranking will be occupied by *d*, *e*, and *f* in some order. To determine which order, it suffices to apply Condorcet's criterion to these three alternatives alone; that is, they should be ordered so as to agree with the maximum number of pairwise votes that they receive *among themselves*. The reader may verify that the ordering that accomplishes this objective is *dfe*. Hence the total ordering implied by Condorcet's rule is *abcdfe*. This is quite different from the Borda ordering.

I have worked out the details of this example to show that Condorcet's rule is not necessarily difficult to compute even when a sizable number of alternatives is involved (though it is certainly more difficult than Borda's method). It also illustrates why Condorcet's method satisfies a certain form of independence. For example, if the bottom three alternatives are dropped from consideration, then Condorcet's rule (unlike Borda's) must preserve the ordering *abc*. Indeed, if it did not, then the interval *abc* could be rearranged into a new ordering that is supported by more pairwise votes. But then *abc* could also be rearranged *within the full ordering abcdfe* to obtain a new ordering that is supported by more pairwise votes. This contradicts the assumption that *abcdfe* is supported by the *maximum* number of pairwise votes. A similar argument applies if the last two alternatives are dropped, the last four, or indeed any bottom segment of the ranking. Similarly, the ordering of the bottom part of the list will be preserved if we remove the top alternative, the top two alternatives, or any top segment of the ranking.

In general, a Condorcet ranking has the property that the ordering of the alternatives does not change whenever we restrict attention to an *interval* of the full ordering.[15] This property will be called *local stability*. Intervals tend to consist of alternatives that are relatively closely related. For example, an interval might consist of a set of candidates that occupy a certain segment of the political spectrum (e.g., "centrist"). Or it might consist of a series of minor modifications or amendments to a proposed piece of legislation.

I shall show that Condorcet's method is the only plausible ranking procedure that is locally stable. To do so we must recall several properties that are standard in the social choice literature. Let F be a social *ranking rule*, that is, a rule for strictly ordering any set of alternatives given the preference data of the voters. We shall assume that the input data to F is given in the form of a voting matrix: the number of votes for and against each pair of alternatives. All voters are weighted equally because only the *number* of votes counts, not the identities of the individuals who cast the votes. This property is known in the literature as *anonymity*. F is *neutral* if it is not "biased" with respect to one or another alternative: if the names of the alternatives are permuted, then their ranking is similarly permuted. Taken together, anonymity and neutrality require that F sometimes yields ties.[16] A third standard condition in the social choice literature is that F be *unanimous:* if all voters cast their votes the same way on every pair, then the ordering of the candidates agrees with all of the votes.

Finally we shall require a degree of consistency in the way that F aggregates individual opinions into group opinions. Consider a bicameral system such as the U.S. Congress. To pass, a decision must receive a majority of both chambers. This much is clear when a decision involves just two choices. Now consider a decision involving more than two alternatives. Suppose that both chambers use the same decision rule F, and that they consider the same agenda. Let each chamber separately rank all of the alternatives. If both chambers happen to rank them in exactly the same way, then we may say that this ranking is *approved* by both chambers. If a ranking is approved by both chambers, then it stands to reason that it would also be approved if the two chambers were to be considered as a single body (i.e., to cast their votes in plenary session). *F satisfies reinforcement* if whenever a ranking is approved by two separate groups of voters, then it would also be approved when the votes of the two groups are pooled.[17]

The condition of "local stability" can now be formally stated as follows: Let A be an agenda of alternatives, let V be a voting matrix on A, and let $F(V) = R$ be the (unique) consensus ranking. F is *locally stable* if for every interval B of the ranking R, $F(V_B) = R_B$ where V_B and R_B represent V and R restricted to the subset B. In case of ties, the condition says that for every $R \epsilon F(V)$, every interval B of R, $R_B \epsilon F(V_B)$, and if $R'_B \epsilon F(V_B)$ for some $R'_B \neq R_B$, then R'_B can be substituted for R_B in R to obtain a new ranking $R' \epsilon F(V)$.

It follows from a theorem of myself and Arthur Levenglick (1978) that Condorcet's rule is the unique ranking rule that satisfies anonymity, neutrality, unanimity, reinforcement, and local stability.[18] Note that Borda's rule satisfies all of these properties *except* local stability.

Conclusion

The purpose of this article has been to draw attention to a major, but little-known, contribution by Condorcet to the theory of group decision making. Condorcet proposed a decision rule that applies to any voting situation whether cyclic majorities are present or not. This rule can be interpreted as a best guess about the "true" ordering of the alternatives. The rule can also be justified on entirely diffeent grounds; namely, it is unbiased with respect to both individuals and alternatives and it aggregates individual opinions in a "consistent" way. This axiomatic justification differs significantly from the probabilistic one in that it places emphasis on the operational properties of the method rather than on a model about how the world works. Of course one may still question whether these are the "right" properties, just as one may question whether Condorcet had the "right" probabilistic model. But at least the nature of the choice is clarified. Furthermore, one must admit that the specific probabilistic model by which Condorcet reached his conclusions is almost certainly *not* correct in its details. The plausibility of his solution must therefore be subjected to other tests, as I have attempted to do.

I conclude that there is a strong case for Condorcet's rule on two counts. First, in situations where voters are primarily concerned with the accuracy of a decision (as in jury trials, expert opinion pooling, and in some types of public referenda), Condorcet's method gives the ranking of the outcomes that is most likely to be correct. Second, where voters are motivated primarily by their subjective preferences about outcomes, Condorcet's rule is responsive to changes in individual preferences, is unbiased with respect to voters and alternatives, and (unlike Borda's rule) it is relatively stable against manipulation by the introduction of unrealistic or inferior alternatives. In short, we need not resolve the question whether voters attempt to judge which decision is in the public interest or whether they merely register their subjective opinions on an issue. Both of these elements are probably present in most decisions. In either case, Condorcet's method is a rational way of aggregating individual choices into a collective preference ordering.

Notes

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1. Condorcet did not explicitly say that he was addressing Rousseau's problem, but Rousseau's ideas would have been much discussed in Condorcet's circles. Baker (1975, 229) also suggests that Condorcet drew his inspiration from Rousseau. Grofman and Feld (1988) explore the connections between Condorcet's and Rousseau's ideas in greater depth.

2. Poisson (1837) studied a similar set of questions using probabilistic methods. See also Gelfand and Solomon (1973) for recent work in the Poisson tradition.

3. A related problem is to rank teams or players based on their win-loss records (as in baseball, tennis, or chess). Condorcet's model can be used to estimate the most likely true ordering of the players (see Batchelder and Bershad 1979; Good 1955; Jech 1983).

4. The following preference data would yield the pairwise votes in Table 1: six voters have preference *abc*, five voters have preference *bca*, and two voters have preference *cab*. It bears emphasizing, however, that Condorcet did not think of the voters' opinions as expressing their personal preferences. Rather, they represent the voters' best judgments as to which candidate in each pair is the better one.

5. In this translation I have modified the mathematical notation slightly to conform with modern conventions.

6. Michaud (1985) comes to a similar conclusion.

7. I exclude the case $p = 1$, because this would imply that all voters agree with probability one. The case where the voters have different a priori probabilities of being right is treated in Young 1986 (see also Nitzan and Paroush [1982] and Shapley and Grofman [1984]).

8. For reasons that remain obscure, Borda's method was jettisoned by Napoleon after his own election to membershp (Mascart 1919).

9. In a footnote, Condorcet adds that his own treatise "had been printed in its entirety before I had any acquaintance with this method [of Borda], apart from the fact that I had heard several people speak of it" (1785, clxxix).

10. Condorcet arrived at this answer intuitively, not by an explicit application of Bayes' rule.

11. This "minimax" rule was proposed in modern times by Kramer (1977).

12. Borda himself gave this alternative statement of his method.

13. This idea was conjectured independently by Grofman (1981). The distinction between the majority alternative and the alternative most likely to be best has also been discussed by Baker (1975) and Urken and Traflet (1984).

14. The following argument was suggested by Bernard Grofman.

15. This property of Condorcet's method (which is known in the literature as the "median procedure") was noted by Jacquet-Lagrèze (1969). For other results on the median procedure see Michaud and Marcotorchino (1978), Barthelemy and Monjardet (1981), Michaud (1985), and Barthelemy and McMorris (1986).

16. For example, if there are an even number of voters and every pairwise vote is a tie, then anonymity and neutrality imply that all linear orderings of the alternatives are equally valid. Thus F must sometimes take on multiple values.

17. We require furthermore that if another ranking is approved by only one of the two groups, then it is *not* tied with a ranking that is approved by both of them.

18. Let F satisfy the five conditions of the theorem. When there are exactly two alternatives, it follows that F is simple majority rule (Young 1974). Now consider the case of more than two alternatives. Let R be the social order. Local stability implies that whenever two alternatives are ranked one immediately above another in R, then their ordering remains invariant when F is restricted to those two alternatives alone. Since F is simple majority rule on every two alternatives, it follows that in the social order R every alternative must defeat (or tie) the alternative ranked immediately below it. Furthermore, if the two alternatives are tied, then they may be switched to obtain a social ordering tied with R. It can be shown by induction on the number of alternatives that this property, together with neutrality, unanimity, and reinforcement implies that F is Condorcet's ranking rule (Young and Levenglick 1978).

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HISTORY AND DISCIPLINE IN POLITICAL SCIENCE

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Once sparce and sporadic, histories of political science have proliferated in recent years. We contend that such histories are a necessary feature of the discourse of political science, because there are essential connections between the history, identity, and actual practices of any rationally progressive discipline. In light of the fact that the objects political scientists study are historically and contextually contingent, there has been—and should be—a plurality of histories to match the diversity of approaches in political science. Unfortunately, most histories of political science prove either "Whiggish" and condescending toward the past, or "skeptical" and negative. The consequence has been an inadequate understanding of the relationship between plurality, rationality, and progress in the discipline. Taking into account both the deficiencies and achievements of Whiggish and skeptical accounts, we argue that context-sensitive histories would better serve the rationality and progress of political science.

> Who controls the past controls the future.
> Who controls the present controls the past.
>
> —George Orwell, *1984*

The occasion of this essay is a recent plethora of works attending to the history of political science. Recent additions to what was once a sporadic and discontinuous genre include books by Blondel (1981), Collini, Winch, and Burrow (1983), Higgott (1983), Kavanagh (1983), Natchez (1985), Janos (1986), Ricci (1984), and Seidelman and Harpham (1985) and shorter pieces by Riker (1982), Keohane (1983), and Gunnell (1983).[1] On reading these works one is struck by the variety of conclusions drawn regarding the discipline's past and how it shapes the present state and future promise of the discipline. Our intent is to ask what, if anything, disciplinary histories can contribute to the identity, practice, and progress of political science.

Our account is designed to enable political scientists to parse disciplinary histories for positive and negative lessons. The foundations of this account, however, require that we say something about the relationship between disciplinary history and identity, for two related reasons. In the first place, it may not be obvious—especially in light of the variety of conclusions they have drawn—that disciplinary histories can provide practitioners with any guidance for their research. We shall argue that they can and indeed that such history is an ineliminable feature of any account of the discipline's identity. This connection arises not just in terms of an intimate relationship between the history and philosophy of science—two forms of commentary on the practice of science. We shall argue that in political science more than elsewhere there is an essential link between disciplinary history and the actual practice of inquiry. And if this first thesis can be sustained, standards of judgment for adjudicating competing histories must also be articulated.

We follow the lead of "postempiricist" philosophy of science to argue for the centrality of disciplinary history. This claim is both prescriptive and descriptive, for the writing of disciplinary history is not limited to the texts we cited at the outset but rather pervades the discourse of political scientists. The purpose of this section is thus to establish the importance of disciplinary history and its place in the actual practices of political scientists.

The utility of such histories in understanding the identity of political science is, however, a different question. One of the difficulties in establishing standards of judgment for adjudicating disciplinary histories is that our histories have a character different from those in the natural sciences. Whereas the natural sciences generally enjoy hegemonic histories to which their practitioners subscribe, political science has multiple histories. This difference turns out to follow from the socially constituted and historically determined nature of the objects of political inquiry. Thus an adequate understanding of the relationship between history and identity in political science requires an appreciation of the historically and contextually contingent character of disciplinary practices.

In the final section we flesh out the implications of the preceding arguments by pinning down the contributions disciplinary history has made—and could make—to the progress of political science. This exploration is carried out by examining actual histories of the discipline. We shall argue that for all their virtues, these works have often distorted the history and identity of the discipline. Virtually all of them are either "Whiggish" and overly condescending toward the past, or "skeptical" and excessively dismissive. We build on our criticisms to argue that political science has a past whose achievements can be vindicated and a promising future—but only if political scientists are willing and able to write disciplinary histories that do full justice to past contexts.

Disciplinary History and Disciplinary Identity

We began this essay by noting that there have been a number of self-consciously historical studies of the discipline published in the last few years. Several of the authors' aims are clear enough: they intend to provide not only a description of the past but also a set of prescriptions for the discipline's future (the one exception being Collini, Winch, and Burrow 1983). The past rarity of such sustained treatments should not obscure the fact that political scientists have frequently deployed historical reconstructions in arguments about the discipline. To test our contention the reader need only consult any collection of "scope and methods" texts. Most of them—and we found few exceptions—include some discussion of the history of political science, deployed to warrant a particular methodological stance. Or consider the review essays that periodically grace the professional journals, APSA presidential addresses on the state of the discipline, the texts introducing students to a subfield, and the numerous collections of essays on the state of subfields. All of these use historical reconstruction to warrant some particular perspective.

The same has always been true of political philosophy. So, for example, Aristotle felt the need to wrestle with Plato (e.g., *Nichomachean Ethics* 1095a32); and Machiavelli with those who "imagined republics and principalities which have never been seen or known in reality" (Machiavelli 1950, 56). Even

Hobbes—that most antihistorical of thinkers—felt he had to engage "the philosophy schools, through all the universities of Christendom, grounded upon the texts of Aristotle, [which] teach another doctrine" (Hobbes 1962, 22).

Our point in noting these uses of historical reconstruction is that disciplinary history in political science, as in other fields, is generally used to legitimate a particular perspective while delegitimating competing approaches (see Lepenies and Weingart 1983, xv–xx). Such a conclusion does not sit well with some historians. Most notably, Collini, Winch, and Burrow (1983) bemoan the insidious effects of such uses of disciplinary history. Their own work stands out from the other histories now available for a number of reasons. First, the authors are not political scientists, but rather self-styled intellectual historians. Second, their concern is with a somewhat distant episode, "the aspiration to develop a 'science of politics' in nineteenth-century Britain" (p. 3). But more significant for our purposes is that the authors proclaim an intellectual "aversion to discipline history" of the sort that would provide "nourishment or some other form of comfort" for present-day practitioners (pp. 4, 7) and so attempt to sever the connection between disciplinary history and identity. In fact, they deny that theirs is a *discipline* history at all (pp. 4–5).

As they note, legitimating histories are often informed by "the present theoretical consensus of the discipline, or possibly some polemical version of what that consensus should be" and as such "reconstitute" the past "as a teleology leading up to and fully manifested" in this consensus. Consequently, "the intellectual map of the present, or some version of it" is superimposed on the past, thereby obliterating the interests, concepts, categories, and self-understandings of past figures (pp. 4–5).

There is more than a kernel of truth to this criticism of what the authors call Whiggish history (p. 5). Others have also noted the ubiquity of such histories in the social sciences (see Bernstein 1976, 97–98; Lepenies and Weingart 1983, xiii).

It is noteworthy that the attempt of Collini, Winch, and Burrow to sever disciplinary history from current disputes about the identity of political science is belied by their own work. While insisting that they "remain agnostic on . . . fundamental and ultimately epistemological problems" (p. 7), they do in fact pass judgment regarding such questions on their subjects (Dugald Stewart, Malthus, Bagehot, Sidgwick, etc.; see, e.g., p. 376).

Now it might be objected that in fact these judgments are merely contingent features of their work: remove them and one is left with a history having no partisan intent. The difference between the approach advocated by Collini, Winch, and Burrow and the approach of the political scientist interested in disciplinary history would then be that the former wish merely to explicate the self-understandings of past figures while the latter must care about the adequacy of those self-understandings.

This distinction, however, collapses on closer scrutiny. "Intellectual history," Rorty, Schneewind, and Skinner (1984, 9) argue, "cannot be written by people who are illiterate in the culture of their prospective readers. . . . To put present day readers in touch with a past figure is precisely to be able to say such things as 'This was later to be known as . . .' and 'Since the distinction between X and Y was yet to be drawn, A's use of "Z" cannot be interpreted as . . .' But knowing when to say such things—knowing what to bracket when—requires knowing what has been going on in all sorts of areas."

In other words, the very possibility of writing about something that would count as an episode in a discipline presupposes some understanding of its identity. Indeed, one need only read the introductory chapter of their book to recognize the extent to which Collini, Winch, and Burrow actively "bracket" their subject for, or make it intelligible to, contemporary practitioners. This task requires some understanding of what these practitioners actually do (and should) mean by political science.

Our criticism of Collini, Winch, and Burrow is not meant to disparage the substantive content of their historical reconstruction but simply to stress that there can be no nonlegitimating or neutral stance from which a disciplinary history can be written. All such histories will be selective, and guided by some commitment (or opposition) to a particular identity.

But if disciplinary history must always be written from some such perspective, might it not follow that we really have no need to tarry with the problem of which histories to accept as adequate? Here, it might be argued that to write the history of the discipline is one thing, to actually do political science quite another. On this objection, at least, intellectual historians such as Collini, Winch, and Burrow and the political *scientist* might well agree—at least to the extent that both would like to sever the connection between history and identity.

And indeed, for their part, political scientists have often treated the discipline's past as if it were a history of "prescience" or "ideologies" or "philosophy," with the present (or imminent future) bearing witness to the emergence of a real science. It is not only twentieth-century practitioners who have tendered this line. Since Hobbes (at least) it has been widely argued that social and political science is a possibility if not a reality. At the root of these arguments one finds a common belief that science proceeds by following a particular method. Hobbes' method was one of thinking of "men as if but even now sprung out of the earth, and suddenly, like mushrooms, come to full maturity without all kinds of engagement to each other" (quoted in Lukes 1968, 119). Most modern practitioners place their faith in the methodology of the natural sciences.

If these methodological aspirations could be realized, they would clearly vindicate those political scientists who read the history of the discipline in terms of a modern break from a prescientific past. On this account, the standards against which the materials generated by the historian are to be judged would be provided by the scientist. Moreover, the identity of the discipline would be independent of its past, a methodological not a historical matter. The history (or better, prehistory) of political science would have interest only as a source of examples of attempts to articulate a scientifically grounded knowledge of politics.

Unfortunately for its advocates, this position is now discredited among philosophers of science. The undermining of this "method fetishism" (Putnam 1981, 188) had been in the offing for some time (see Manicas 1987, 241–44) before the death blow dealt to it by Kuhn (1970). Kuhn's work served as an important catalyst in the development of post-empiricist philosophy of science (Hesse 1980, 167–86; Bernstein 1983, 20–25). The virtually unanimous conclusion of the postempiricists is that the rationality and progress of science do not depend on conformity to the "logic of scientific inquiry," whether positivist, logical empiricist, or Popperian. As Kuhn (1970, 200) put it, "There is no neutral algorithm for theory choice, no systematic decision procedure" for determining the "scientific" status of a knowledge claim. Or as MacIntyre (1977, 468) argues, "There is no set of rules as to how science must proceed and all attempts to discover such a set founder in their encounter with the actual history of science."

MacIntyre's comment returns us to the history-identity connection. Against the apparent relativism implicit in a Kuhnian perspective, more recent postempiricist philosophy of science has tended to emphasize that the rational status of scientific theories is secured in their historical development (Burian 1977, 30). To evaluate a theory, or set of theories, or even a methodological and metatheoretical stance is to write a particular sort of history in which the stance in question is seen as rationally superior to its competitors, past and present. Following Lakatos (1970), stances "are to be assessed by the extent to which they satisfy historiographical criteria; the best scientific methodology is that which can supply the best rational reconstruction of the history of science and for different episodes different methodologies may well be successful" (MacIntyre 1977, 469).

Lakatos' claim that "philosophy of science without history of science is empty; history of science without philosophy of science is blind" drives home the point (1978, 102). Not only is disciplinary history going to be articulated from a particular account of identity, but any identity must be grounded in a disciplinary history. Disciplinary history and prescriptions for identity are properly understood as but two moments in the same reflective process.

It follows from these claims that "how we judge the status of a science depends on how we judge the quality of the history it assists in providing" (MacIntyre 1984, 44). This turns us toward the question of the kind of history that can underwrite the rationality and progress of political science. An answer can be approached by examining the differences between the history of political science and that of other rational disciplines—especially the natural sciences.

Hegemony and Plurality in Disciplinary History

All disciplines go through occasional upheavals during which competing orientations (metatheories, paradigms, research programs, or research traditions) vie for supremacy. The proponents of any new view must, of course, argue that their program is superior. But if the postempiricists are right, this superiority cannot be one of *method.*[2] Rather, rational superiority must be established through the writing of a disciplinary history in which it can be shown that one program provides an account that is able to explain both the successes and failures of its extant competitors (MacIntyre 1984, 43).

This superiority, however, must be established not only over the current received wisdom but also with respect to all orientations that have previously held prominence in the discipline (MacIntyre 1984, 44–45). In other words, the Young Turks, or at least some of their number, must do disciplinary history of two related sorts. In a narrow and foreshortened sense, they need to demonstrate (following Lakatos) that their extant competitor has been running out of steam with time in terms of its ability to solve problems or predict novel facts. Such arguments, however, also presuppose a longer narrative in which all past approaches (i.e., prior to the current competitor) are safely dead and buried.

On most accounts, the contemporary natural sciences exemplify this kind of progressive history. Their practitioners may safely assume that their history consists of the effective supercession of less- by more-fruitful research traditions. Thus they need not consider resurrecting anachronistic research traditions to further their science. For example, in evolutionary biology, today's punctuated equilibrium theorists quite reasonably feel little need to trouble themselves with pre-Darwinian understandings. As Kuhn (1969, 407) argues, "Science destroys its past."

It can, then, be argued that the contemporary natural sciences enjoy a hegemonic (albeit multifaceted and complex) history and identity. And disciplinary history can figure in natural scientific progress to the extent of its ability to identify the rational warrants of paradigm shifts. This kind of contribution can also apply in political science, but there is an important difference to be taken into account. For in political science we lack general agreement on who counts as an important figure and what counts as a progressive development. Instead, we allow numerous competing accounts of the intellectual movements, research programs, traditions, and central figures that have shaped (or on some accounts misshaped) the discipline. We have a variety of histories to match our plurality of identities.

This contrast with natural science might be taken as grounds for arguing that political science is "immature." But such a claim ignores the particular features that give rise to our disciplinary plurality. Unlike natural science, political science *cannot* "destroy its past." "Global" progress, at least in the commonly understood terms of linear succession of increasingly successful research traditions, cannot occur in political science. For the empirical problems and conceptual disputes that are the grist for comparative judgments across research traditions are themselves historically specific and socially constituted. Thus any computation of a standings table of different traditions is contingent upon time, place, and a particular set of empirical and conceptual problems. To take a crude example: behavioralist approaches seem to work well in the apparently placid fifties but are severely strained by the turbulent sixties. Resurrection of apparently obsolete research traditions is also possible if socially constituted empirical problems shift in a direction that gives them renewed problem-solving power. So Marxism returns along with protest and conflict in the industrial world, and an obvious lack of development in much of the third world.

In this light, the progress of political science consists of an expanding capacity to cope with contingency in the content of empirical problems (Dryzek 1986, 315–17). Thus political science is always *rationally* going to be home to a variety of research orientations. Any approach trying to demonstrate its utility or superiority must do so not just through reference to the extant range of traditions (and the history of each) but also in relation to past traditions. Every approach needs its historians, to write (or rewrite) at least some of the discipline's past.

However, such histories will be limited in their scope, for, as we have already intimated, the "objects" of political science—unlike those of the natural sciences—are constituted by the beliefs and self-understandings of social agents, among whom may of course be numbered political scientists themselves.[3] The very existence of these objects—say, a bureaucracy, army, monetary system, political party, monarchy, capitalist economy, socialist state, or democracy—is contingent on the subscription by social agents to some particular beliefs or theories. There is no analog to this in the natural sciences, where the objects—however they may be conceptualized—are not constituted by theories. They exist quite independently of whatever the scientist may *say* about them (Hacking 1984, 115, 116). Moreover, in political science, as agents' "theories" change, the objects will be altered, come into, or go out of existence. Add to this the post-empiricist claim that theories are "underdetermined" by evidence (Hesse 1980, 32, 144, 187), and you have all that is needed for a radical plurality of research traditions, hence of disciplinary histories and identities.

What you also have, however, is the groundwork for the special centrality of disciplinary history in political science. In this regard, political science resembles philosophy more than it does the natural sciences. For to develop any creative alternative to dominant current understandings—arguably the essence of philosophy—the philosopher has to engage in "genetic" spadework of the sort that would explain why some orthodoxy came to be victorious and how and why it is vulnerable (Taylor 1984, 19). In so doing, the philosopher can warrant the superiority of his or her favored alternative not just through reference to the extant range of traditions (and the history of each) but also in relation to past traditions.

This need to do an in-depth history of a discipline like philosophy or political science is most obvious in those cases where some orthodoxy is being challenged (Taylor 1984, 21–22).[4] Young Turks, in other words, are less privileged than their orthodox opponents (whose successful battles with other traditions are often safely behind them) and must work harder at their history.

Sometimes the Young Turks may even have to pin down the identity of the orthodoxy they are challenging. The hard work of reconstruction of a rival is exemplified by Seidelman and Harpham (1985), whose history confirms the demise of what they call "third tradition" U.S. political science. This tradition begins with Ward, ends with Lowi, and along the way is host to Woodrow Wilson, Bentley, Beard, Merriam, Lasswell, Key, Truman, and Burnham. According to Seidelman and Harpham, the third tradition is defined by its use of the tools of science in democratic reform of a sick but not incurable U.S. polity. The leading figures in this third tradition were doubtless unaware of their common membership, and lacked even a name until Seidelman and Harpham came along. In what sense, then, do we have a real tradition here? The answer is surely that a coherent tradition of such reform science makes most sense in terms of a competitor to the more radically anticapitalist agenda for political science to which Seidelman and Harpham seem to subscribe. Here one can readily see the legitimating function of disciplinary history, for the very idea that there is an extended and coherent third tradition of reformism is of interest primarily to those who seek an alternative to it. On Seidelman and Harpham's own account, those who practiced and extended the tradition did so without benefit of the kind of history these two authors have now written. So the most logical approval for Seidelman and Harpham would come from leftists like themselves, or possibly antireformist conservatives.[5]

An equally dramatic and ambitious retelling of a hundred or more years of U.S. political science is offered by Ricci (1984). To Ricci this history is a tragedy in which a desire for scientific respectability has muffled the contributions the discipline could have made to the great conversation of democratic development. Thus Ricci reconstructs the last century of U.S. political science in terms of an enterprise that is avowedly and self-consciously scientific from the outset. Again, the legitimating function of disciplinary history is apparent. Ricci's narrative of tragedy makes sense only in contrast to his own agenda for the discipline, which involves more in the way of a focus on the key concerns of democratic politics, speculative thinking, and grand theorizing. However, Ricci's reconstruction would probably make no sense to behavioralist disciplinary historians. For example, to Easton (1953, 4–7) the main problem with U.S. political science in the first half of the twentieth century was its *repudiation* of scientific reason. Where Ricci sees only a lamentable obsession with science, behavioralists see only an equally lamentable absence of science.

The need to proclaim research agendas through reference to intellectual history applies not just at the grand disciplinary level of Seidelman and Harpham and Ricci but also in the subfields of political science. Thus Natchez (1985) writes a history of opinion research to demonstrate that nobody (partially excepting V. O. Key) has studied voting behavior from the angle of the liberal constitutionalist political theory that Natchez believes should be the wellspring of this research. And Keohane (1983) justifies his self-consciously Lakatosian "modified structural realist program" for international relations through references to the achievements and failures of realists from Thucydides to Morgenthau and beyond.

However important disciplinary history is for Young Turks, the defenders of orthodoxies have to write it as well. One reason for this is that given the plurality and disunity of the discipline, "orthodoxy" in political science is a rather tenuous and fleeting condition. So even a widely accepted understanding of disciplinary identity needs it historians in order to demonstrate its own historical coherence—that it really is a paradigm, research program, or research tradition worthy of the name. Equally important, every approach must fend off challengers by demonstrating the expansion of its explanatory or problem-solving capabilities over time. Thus Riker's (1982) interpretation of history of Duverger's Law is designed to bolster the rational choice research tradition, not to mention a positivist understanding of our history. To this end, Riker relates not just the refinement of theory through its confrontation with (what he takes to be) the eternal verities of political life and party systems but also the impact upon theory of events like the advent of a single party in India's plural voting system. Riker's interpretation proceeds with the avowed intent of demonstrating that what he calls political science but what we may call the rational choice research tradition does in fact have a history that is both coherent and progressive in terms of explanatory power.

While we could multiply examples here, our general point should by now be clear enough. As long as there is variety in orientations toward political science—and this condition is to be fully expected as long as there is history and politics—then there will be variety in disciplinary histories. However, it does not follow that one can write a disciplinary history in any way one chooses, nor does it mean that all disciplinary identities are created equal. We now turn to the question of how one might separate the wheat from the chaff in both histories and identities.

The Limits and Lessons of Disciplinary History

Having established the importance of disciplinary history for our identity, we can appreciate the efforts of those who have engaged the difficult task of addressing this history and applaud their contributions to the rational pluralism of the discipline; for if our arguments are right, then it is only by judging the quality of these histories that practitioners can determine the merits of the research programs they presuppose. But this also means that any appreciation and applause must be tempered by a critical interrogation of these histories. We shall now attempt to advance some critical standards for disciplinary historians and practitioners alike.

Our critique begins by noting that disciplinary histories are necessarily linked to agendas for disciplinary identity, as our earlier discussion of Collini, Winch, and Burrow sought to demonstrate. This link is at once a strength and a weakness. It is a strength because it provides both a reason and a vantage point for historical reconstruction. Without this connection, a history could hardly be a history at all; devoid of an intelligible point of reference, it would itself be rendered unintelligible. Readers of such an imaginary history would lack the conceptual resources needed to make sense of the story at hand.

But the link is also a weakness, for any history so informed is going to be partial and limited in some potentially debilitating ways, especially when one considers the extreme variety in political science approaches—and hence in disciplinary histories. No Archimedean point is possible here. Disciplinary historians, no less than other social agents, are limited by the contexts from which their own understandings arise. They cannot possible encompass the whole range of contemporary—let alone past and future—perspectives.

Variety in political science is a mixed blessing. While we have argued that it is both rational and necessary, unsympathetic observers of the discipline might charge relativism and a lack of global progress. To make matters worse, disciplinary history as currently practiced might well lead one to conclude that political science is without past achievement. Most of the extant histories are at best condescending toward past practitioners. Often they are outright condemnations. This is particularly true in those cases where it is modern political science that is in question (although still true in the longer history of political thought). If these histories are taken to heart, our past would hardly seem to constitute proud, happy, and progressive disciplinary development.

Our own more sympathetic view is that properly understood, such histories can indeed provide reasons for the adoption of one orientation or the rejection of others and so the stuff of a progressive discipline. But as yet that promise remains unfulfilled, for reasons we shall now explain.

Virtually all of these histories fall into two mutually exclusive categories. On the one hand we have those historians who are just as Collini, Winch, and Burrow described them: *Whigs.*[6] On the other there are those best described as *skeptics.*[7]

Whigs write the history of the discipline as though it were teleological, culminating in some correct current understanding or, more usually, some bright imminent future in which past difficulties are resolved. For them the past has little intrinsic value. Indeed, their inspiration stems from the ideal of liberating the discipline from the dead weight of past errors and illusions.

Our Whigs are legion. To begin over 50 years ago, Merriam (1925), with a little foreshortening, classified the history of political science into four increasingly powerful stages. The first was a priori and deductive, ending in 1850; the second was historical and comparative, dominating the second half of the nineteenth century; the third involved observation and measurement, entering with the twentieth century; and the fourth, just beginning in 1925, was to stress psychological analysis. Only in this fourth, highest, stage would true science arrive. Merriam repudiated his immediate predecessors such as Beard and Bentley for their want of science.

Later, the behavioral revolutionaries would see only intimations of real science amid the supposedly dominant hyperfactualism and institutionalism of their forbears. But few among the revolutionaries believed they were starting with a blank slate. Selected precursors like Bentley and Merriam could be congratulated. Easton (1953) pays homage to a long line of thinkers from Aristotle to Bentham and beyond, even as he believed the momentum toward a science of politics was interrupted in the twentieth century.

Later still, the Kuhnian moment in APSA presidential addresses announced the arrival of the first paradigm (e.g., Almond 1966). More recently, the contemporary "science of political science" has been celebrated by Weisberg, who (even while admitting he has no idea what *science* means) asserts that "political science has become more scientific over the years" (1986, 3, 10). Blondel (1981) and Kavanagh (1983) are equally pleased with the momentum of post-1945 political science in comparison to its primitive past. Within public administration, Miller and Moe (1986, 167) announce the arrival of the "positive theory of hierarchies" to rescue the subfield from its "lack of theoretical progress."

Whigs generally restrict their approval to a few precursors. For example, Riker (1982) takes pains to identify the individuals who, over a span of more than a century, have anticipated, stated, or refined Duverger's Law. Yet Riker's approval of these people is for the most part lukewarm, for it is only in the last three decades that the law has been taken under the wing of rational choice theory, and thereby "examined with increasing scientific sophistication" (Riker 1982, 765). For him, the past merits congratulation only for its intimations of a glorious present. Riker does, of course, recognize that alternative disciplinary identities have been advanced by those outside the rational choice tradition. But beyond cursory denigration of "belles lettres," Riker is aware of no serious rivals. That is the privilege of those who subscribe to orthodoxies.

Some Whigs are more charitable still in ascribing to their predecessors the possession of paradigms—albeit partial and flawed ones. If the Kuhnian terminology is taken at face value, these precursors thereby have scientific rather than prescientific standing. Thus Janos (1986) portrays a switch of paradigms in the study of political development from the "classical" approach of Smith, Comte, Marx, and Weber to contemporary cultural theorists of postindustrial society. Whether charitable or not, however, Whiggish histories treat the disciplinary present and future in terms of triumph over the limited perspectives of past practitioners.

In contrast to our Whigs, skeptical historians find little to commend in the present and still less to approve of in the modern history of the discipline. Skeptics write the history of political science in terms of unremitting error. So Ricci (1984) laments the results of the persistent efforts of the discipline to be a science just like all the others. Gunnell (1983) condemns contemporary political theorists for retreating from real political concerns and intelligible language into abstruse metatheory and incomprehensible idioms. Natchez (1985) laments the all-pervasive atheoretical bent of voting studies, from beginning to end. From an earlier era, Crick (1959) is none too pleased with the *Americanized scientism of politics* (see also Storing 1962). Remember, too, Bentley's denunciation of the "soul stuff" of his predecessors and contemporaries; though, unlike Crick, Bentley wanted to turn toward, rather than away from, science. Seidelman and Harpham (1985) are somewhat more sympathetic than these other skeptics. Though in their eyes the third tradition ultimately fails, the very reason they need to take it on lies in its victory within the discipline over the first (institutionalist) and second (radically democratic) traditions.

The skeptics are the Whigs' obverse; they conclude that the redemption of political science requires that it abandon its recent past entirely. Those we have noted hold to a common view that what should succeed the jettison of our past is a turn to practical political concerns with a view to becoming relevant to society and its problems. To Seidelman and Harpham this turn would involve a radical agenda. To Natchez this same move would be conservative: he believes that liberal constitutionalist political theory should guide voting studies precisely because it is the dominant political tradition in the United States. For Ricci this turn is to be accomplished by moving beyond the apolitical nature of contemporary political science and theory. Bentley attacked "soul stuff" for the sake of a science committed to democratic reform. One can imagine skeptical histories not committed to political relevance, but we can locate none.

Between these two stances there would appear to be no others. Nor should we be surprised by this: Whiggism and skepticism are simply historiographical expressions of the contemporary rifts in the discipline of which most of us are—sometimes painfully—aware. But must we consort with either the Whigs or the skeptics? We think we need not consort with either but to see why we need to be clear about the errors—and strengths—of both stances.

As we have suggested, Whiggishness is defined by its attempted justification of a particular disciplinary present (or near future). This present (or imminent future) may in its turn be defined by present approaches, which are presumably proving themselves by their ability to cope with empirical and conceptual problems as defined and weighted by both present practitioners and the larger society in which they move. However, once we recognize that these present problems are themselves historically contingent and socially constructed, the Whigs' accounts of the past become suspect. For Whigs fail to allow that past practitioners could not have even conceptualized our problems, for they had their own problems to address. As we noted above, research orientations wax and wane under the influence of changes in socially determined and historically specific problems. So the "superior" understanding of the present is itself historically contingent—and likely to meet condemnation from future Whigs for its failure to contribute to the resolution of *their* problems.

Consider, in this light, Dahl's (1961, 279, 280–81) claim (as criticized by Ball 1983, 36–37) that modern opinion research has falsified Aristotle's assertion that humans are political animals. Aristotle, of course, could not have even conceptualized the idea of most people failing to take advantage of minimal and constrained opportunities to participate in a governmental system that would not even merit the name of *politics* in his terms. Quite simply, humans are indeed *zōon politikon* in the terms of Aristotle's own milieu, however much they have become "apolitical" in most modern societies.

All this is not to say that the Whigs are without achievement. For they do engage in what we have argued is the essential task of reconstructing research traditions, thus providing historical warrants for the triumph of their favored approaches. Yet the Whig's historical ledger is distorted. The positive side of that ledger represents similarities between past problem contexts and present ones, making particular past practitioners look like precursors of the present and present insights useful for understanding past circumstances. But as the present changes, so will its precursors, and Whigs will have to continue rewriting history. Thus Bentley comes to prominence as a precursor of pluralist political science in the 1950s—and fades along with pluralist interpretations in the 1960s and 1970s.

In all of this, then, the self-confidence of Whiggish political science turns out to be unjustified. Today's Whigs do not necessarily possess a better understanding of politics than past practitioners. Instead, they may just have a better understanding of a particular kind of politics. Political change may render older conceptions anachronistic, but the claim that we are getting closer to some timeless truth as a result is unwarranted. The Whigs are bad historians because they miss the contextual dimension of inquiry. Their sweeping claims about political science will no doubt look as silly to future Whigs as past Whigs look to them.

The skeptics' errors are in a sense the obverse of the Whigs. Where Whigs see nothing but brightening skies the skeptics see nothing but ever darkening ones. Their conclusion that the history of modern political science provides little worth salvaging would be justified if two arguments could be carried. The first is the argument that over the period of interest some exclusive approach has dominated the discipline as a whole, or at least some well-defined subfield of it. The second, contingent upon the first, is the argument that this orthodoxy has encountered a crisis of sufficient magnitude finally to extinguish its claims upon the discipline. So, for example, Seidelman and Harpham (1985) argue that the third tradition has dominated U.S. political science. This tradition expires in the hands of Burnham and Lowi, for—besides advocating particular reforms rooted in their science—these authors use their science to explain why such reforms can never make any headway in the U.S. polity (Seidelman and Harpham 1985, 221).

The skeptics' first argument is overstated: there is no hegemony, and there never has been. However dominant institutionalism or behavioralism or the Michigan model or realism may have been in their time, they have never been exclusive. Disciplinary pluralism is the norm, and the existence of skepticism itself accentuates that pluralism.

The second argument typically carries with it the charge that political science has always been—or has become—sundered from the hard and interesting questions of real politics. But the skeptics themselves and the occasional figures for whom they express some sympathy (as, for example, Natchez [1985, 183–210] pays homage to V. O. Key) have clearly revealed much about real political issues. More important, our skeptical historians show that even those they attack have made important contributions.

In this light, consider Seidelman and Harpham's (1985) portrayal of certain *"third traditionists."* According to them, Lowi has explained why the reality of interest group liberalism is so resistant to reform (p. 201). Lasswell did indeed uncover many of the irrationalities of mass and elite politics (pp. 134–37). Bentley's "muckraking" science did indeed expose many of the illusions of U.S. politics held by the public and reinforced by political scientists (pp. 69–81).

Consider, too, some of Ricci's (1984) villains. Even as he disparages the "Popperist" foundations of midcentury political science, Ricci admits that Popper's grounding of liberal democracy in a model of scientific inquiry does indeed provide a rationalistic defense of democracy against its totalitarian rivals exactly when such a defense is needed—in the 1940s (pp. 119–25). And Ricci recognizes that the behavioralist voting studies he disparages did tell us some interesting things about politics—if only that Western electoral democracy cannot flourish unless both elites and masses are ignorant of the voting studies findings (p. 175). Natchez (1985) inadvertently demonstrates that the voting studies have all along been corroborating liberal constitutionalist claims about the limited capabilities of the ordinary person, even as the opinion researchers themselves, in their theoretical ignorance, were unaware of this fact. And Gunnell (1983) admits that political theorists have not been completely isolated from the political realities of their time, even if they have not become engaged in the ways Gunnell would prefer.

Like the Whigs, our skeptics fail to do justice to success in context. So Lasswell's accounts of deranged political man are successful in a deranged decade. Popper and Dewey are successful in the context of the midcentury global political struggle. The muckraking science of Beard and Bentley makes sense in the context of a political agenda largely defined by the Progressive political movement. The ambitions for behavioralism of Key and Truman are understandable in a context dominated by a seemingly well-functioning democracy. The Almond and Verba (1963) account of the determinants of stable democracy are successful in a problem context defined by a smug and self-satisfied U.S. political system. And skepticism about the history of political science makes sense in the context of a polity and a discipline that have lost their bearings.

In sum, professionalization has not meant irrelevance to hard political questions. It is professional political scientists who have identified iron triangles, exposed the deficiencies of liberalism, and provided rationalistic comfort for (imperfect) Western democracy in the face of totalitarian challenges.

We can commend the skeptics' recognition of the limits and deficiencies of Whiggish political science. As Gunnell (1983, 38) argues, digging into the discipline's past "demonstrates the inevitability of mortality and the demise of the present." But this does not justify skepticism about the discipline, for the skeptics themselves have shown (just by their very existence) that the discipline is not without redeeming character.

None of this should lead political scientists to scoff at the histories Whigs and skeptics have produced. For it is only by critically examining these—and future—disciplinary histories that we will be able to do better history. Such histories should, above all else, attend to episodes of political science in context. Practitioners, approaches, research traditions, theories, and methods should be apprehended and adjudged for their success or failure according to how well they understood and resolved the problems *they* confronted. In this, disciplinary history can serve—and to some extent already has served—the progress and promise of political science, in two related ways.

At the methodological level, the history of political science demonstrates that the discipline has been—and will continue to be—a historical one. Some methodologies are indeed going to be more or less useful than others. But this determination will be contingent on the ability of a methodology to substantiate its claims historically; that is, by its ability to provide good, hard-headed analyses of political life in particular contexts. Recognizing this would have the meritorious effect of tempering the claims of those who claim to have discovered *the* method of truth, as if this alone were sufficient to establish the progress of the discipline.

The second way in which disciplinary history serves disciplinary progress is at the theoretical level. Political science is about theories of how we have lived, how we do live, and how we can and should live together. And theories are, as the postempiricists have shown, historical entities: they develop over time. But in political science theories are doubly historical in that they are also time-bound (i.e., context-dependent). In this respect, good disciplinary history can serve the double function of enabling "paradigm workers" (or "normal scientists") to be sensitive to the history of their own research programs and to the historical limits of those programs. Given the historicity of political life, theoretical progress in political science will not be of the "vertical"—or transhistorically successive and successful—kind found in the natural sciences. Instead, our progress can only be a "lateral" accumulation of potentially useful research traditions, each of which is contextually constrained in its problem-solving power (Dryzek 1986). It may be the case that some long forgotten or apparently outdated research program will shed light on newly encountered problems or even old ones that remain unresolved. It may even be the case that in order to understand the deficiencies of a theory we need to recover its history to find out why, how, and whether it can still be useful or should be abandoned. Thus good disciplinary history can improve our abilities to make good, contextual choices by making available a varied menu of alternative approaches to our subject matter, along with evidence about when each tradition is likely to be useful, and when it is likely to fail or be irrelevant.

We conclude that there is no neutral stance for evaluating, accepting, or rejecting disciplinary identities. Rather, standards can only emerge in the conflicts and debates within and between traditions of inquiry.[8] It is in this conflict and debate that the relationship between disciplinary history and identity crystalizes, as we hope to have shown in our discussion of Whigs and skeptics. In this respect, plurality is going to be the essence of, rather than an obstacle to, the progress of political science.[9] But complacency does not follow from this approval of pluralism. We should be vigilant in our criticisms without being dogmatic, hardheaded in our inquiries without being intolerant of differences, and vigorous in the development of our own positions without being parochial. Such attitudes result from recognizing the historical and contextual situation of perspectives held by those with whom we disagree—and by ourselves. Good disciplinary history both reflects these attitudes and cultivates them. What remains, therefore, is to write histories that would sort out the lessons of the past in a way that future practitioners—and publics—might find useful.

Notes

We thank Lee Cheek, David Jacobs, and Dean Minnix for their comments on an early draft of this paper. Terence Ball and James Farr provided much useful critical commentary on a later version.

1. Besides the numerous texts on the history of political thought, earlier efforts include Haddow 1939, Crick 1959, and Somit and Tanenhaus 1967.

2. In the natural sciences, for example, postempiricists have shown that if scientists had actually conformed to one or another particular method, a number of clearly progressive theories and programs would have been strangled (Burian 1977, 39). The same is no less true in philosophy, where "analytical philosophy" has been under attack for its failure to offer a coherent account of its own foundations (Taylor 1984; MacIntyre 1984).

3. For a useful overview of the philosophical issues here, see Bernstein 1976. Disputes about the philosophy of social science have for the better part of a century involved the question of whether the natural sciences can provide us with methodological guidance. Postempiricist philosophy of science has now shown that our methods are or can be the same but that the nature of our objects will determine the knowledge claims we can advance (see Bhaskar 1979, 1986). Isaac (1987) provides an interesting account of the applicability of "scientific realism" to political science. Ultimately, these ontological theses must ride on their ability to adequately reconstruct scientific history. And in this, arguments about the belief- or theory-constituted character of social phenomena can underwrite a better explanation of plurality in the history of political science.

4. Even postempiricist philosophy of science itself needed to go back over the history of science to establish its claims (Bernstein 1983, 73-74).

5. Seidelman and Harpham nowhere admit that they are anticapitalists or leftists, but Theodore Lowi, their mentor, so describes them in his foreword to their book (1985, xvii).

6. The classical critique of Whiggish historiography is, of course, Butterfield 1931. While we do not agree with all of Butterfield's conclusions (our objections can be read through our critique of Collini, Winch, and Burrow), those inclined to Whiggish histories of political science might benefit from Butterfield's account of the mistakes of past Whigs.

7. These categories might be applied to premodern political theory as well, but such an application would take us far afield. In any case, the contemporary examples should be sufficient to illustrate our points.

8. For a fuller development of the implications of this kind of argument as it bears on more general questions of rationality, see MacIntyre 1988, esp. chaps. 18, 19.

9. Thomas (1979) argues for plurality in social sciences in a way that differs from but complements our own. For those who think that plurality is a problem, we recommend Thomas's analysis of Soviet sociology (pp. 180-95), which indicates the sorts of difficulties an exclusive paradigm might entail for political science.

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THE TYRANNY OF REASON IN THE WORLD OF THE POLIS

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The modern language of tyranny has distorted the significance of the Greek term tyrannos. *In ancient Greek the term was accorded to the new ruler in the city, one whose legitimacy did not reside in his bonds to the ancient rulers and ancient families. Tyranny thus suggested a freedom from the past. Reason, as the Greeks understood it, also entailed a breaking away from the physical world. Reason and tyranny thus work together as expressions of freedom, but it is a freedom that in its transcendence of boundaries leads to tragedy. An examination of Sophocles'* Oedipus *draws out both the glory and the failure of the individual attempt of the political actor to rise above the historical particular and the mere body to build a world where reason alone is power.*

The tyrant no longer figures prominently in our political vocabulary. We may talk of the petty tyrant, the child who at age three throws a temper tantrum in the middle of the supermarket and gets the candy bar, but our political terms of castigation refer to regimes totalitarian, dictatorial, or authoritarian. Except for an eccentric few like Idi Amin or Papa Doc, we reserve the term *tyrant* for the private world of power conflicts in the work place or the family.[1] The word *tyranny,* however, deriving from the ancient Greek *tyrannos,* is far richer than the popular image of an individual who abuses power might suggest. Indeed, it uncovers for us the meaning of rule without limits, whether moral, physical, or historical. The *tyrannos* is the new ruler, the one who has come to power in the city by means other than birth or established precedent. Therewith his illegitimacy—but also his freedom.

The ruler as tyrant becomes the paradigm of the free individual, the one self-perceived as unrestrained, able to carry forth thought into deed.[2] Among the ancient Greeks, tyranny incorporated a freedom to break away from what was old and limiting. Released from traditions, the tyrant could transform the world in which people lived. The tyrannical impulse was not necessarily evil but could indicate a creativity and a freedom to transcend the limits inherited from the past. As an escape from the past, tyranny could also become a model for human rationality and theorizing, the capacity to move beyond accepted boundaries and opinions in order to imagine what was previously unimaginable, to transform the world through the power of one's mind and one's speech, severed from the bonds of birth and history. The tyrant and the rational individual are alike in that they both are "unborn." The tyrant, the new ruler, arises irrespective of parentage, free of the limits of biological succession. The goddess of reason (Athene) in the myth springs forth fully armed from the head of Zeus, likewise free of biological generation.

The tyrannical freedom of reason to break away from the past, as the Greeks

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present it, however, elicited tragic consequences for the individual and for the city as a whole. The political task of escaping the tyrant arose not because the tyrant's rule was harsh (often it was not) but because the tyrant's feeling of potency involved a failure to recognize boundaries set by the gods, by nature, and by history. The pre-Socratic philosopher Heraclitus, writing at the end of the sixth century B.C. articulated the general Greek concern with limits: "The Sun will not overstep his measures [*metra*]; if he does, the Furies, the guardians of justice, will find him out."[3] Even the gods must acknowledge limits. But the tyrant and the rational individual feel free to transcend limits. On the one hand the tyrant and the rational individual express our freedom to do and be anything; on the other, they reveal the dangers of such freedom. Many of the Greek tragedies of the fifth century B.C. explore the relationships among freedom, reason, and tyranny as they become manifest in the life of the city.

I will turn to Sophocles' Oedipus as the man who most vividly illustrates the unity between abstract reason and tyranny—and then reveals for himself the tragic limits that history and biology place on the freedom of both. Oedipus' intellect is abstracted from the body (and thus he is able to understand numbers as others could not) and his status as tyrant comes from his apparent distance from the lineage of political authority in Thebes. He rules because he was able to answer the riddle of the Sphinx. Thus his ascent to power on the back of reason is an assertion of freedom that is tragically undermined by the biological basis of his birth. The revelation that his birth, not his reason, is the basis of his claim to rule underlies the tragic uncovering of the play. A political optimism that envisions a world of infinite possibilities, subject only to imagination and reason, meets its match in the last crushing moments of Sophocles' *Oedipus*.

The Oedipus myth itself weighs heavily on us in the modern world. It is a story of passions, sexual and political, of guilt and innocence, of power and disability, of self-flagellation and resurrection, of intentionality and chance. All these surface in the story of the man driven by fate or his own character to kill his father where the three roads meet, to give seed to children in his own mother's womb, and to blind himself when the truth of his own origins is revealed. It is Freud's use of the myth that has colored and, I believe, distorted the tale of the man with the swollen feet. Freud's analysis focuses on the sexual desires inherent in the myth, the private passions that characterize the child's transformation into an adult and thus the family dynamics that take place within the crucible of our most intimate relations. For the Athenian Sophocles the myth of Oedipus is not one of family dynamics. Rather, it sits in the world of the polis, and his play addresses the spectators' lives as public actors.[4] The intrafamilial actions in the tragedy have relevance only insofar as they merge into and give force to the political world of action and order.[5]

Embedded in Oedipus' very name is the knowledge he has and the knowledge he lacks, the sight he at first had and the sight he will lose.[6] The ironic tension implicit in the name is never long absent from the playwright's words, and it sears into the mind of the audience the ambiguous status of human knowledge. Sophocles' *Oedipus* raises the question of the nature of knowledge. What can we know and how can we know it? To what degree is our knowledge based on human calculation and to what degree is it based on our experience of ourselves as physical bodies?[7] Sophocles binds the questions of epistemology to the meaning of rule within the city. To what degree is political action based on the freedom of the intellect, to what degree is it limited by the physical world? Oedipus, as ruler, tragically portrays the limits of human knowledge as against our arrogant assumptions of a boundless capacity for insight. The play in this analysis becomes an exploration of the necessary grounding of power and authority in the direct experience of the world of birth and change. As Heraclitus' Sun is bound by certain measures, so too is human intellect, human power, and human freedom. The tragedy of Oedipus is not the fall of a helpless and faultless ruler or the weakness of man subjected to divine laws but the dashed hopes of the power of the mind to rise above the limits imposed by nature, by our biology, and by our past. It is a tale of boundaries overstepped not because of divine prophecies and a divine world hostile to mortal man but because of the freedom that characterizes the tyrant as the tragic hero.

When Oedipus acquires power in Thebes, he is indeed a tyrant, not the legitimate heir. That place belongs to Laius's son—presumed devoured by the beasts on Citheron—or to Creon, Laius's brother-in-law. Oedipus, however, believes he rules because of his intellect. We know that it is his birth that brings him from Corinth to Thebes and makes him ruler there and thus the perpetrator of the most abhorrent of deeds. But we also know that his intellect—his ability to abstract—saves Thebes first from the Sphinx and then from the plague that his own presence evokes. We learn from Sophocles the political value of Oedipus' abstract knowledge, which saves the city. But Sophocles also raises fears about the potency of human reason and of any rule based on it. Reason and tyranny both entail freedom from the *metra* that even Heraclitus' Sun must follow, but both also meet in tragic confrontation the limits that the past imposes on our actions. As such, Sophocles' play becomes a commentary on the modern pursuit of political and intellectual freedom.

The Tyrant

Who is this *tyrannos* about whom the Greeks speak?[8] We learn from Herodotus that the tyrant is the one who "moves ancestral laws [*patria nomia*] and forces himself on women and kills men who have not been tried" (3.80.5 [1927]). From Thucydides we learn that Alcibiades was considered the potential tyrant: "Most men, fearing the greatness of his lawlessness [*paranomia*] with regard to his body, his daily habits, and the intelligence he showed in whatever he did, set him down as desiring a tyranny" (6.15 [1900]). He manifested such "lawlessness" by breaking the conventions of the society; dressing outrageously in long, purple robes; and having on his shield no ancestral insignia but Eros carrying a thunderbolt (Plutarch, *Alcibiades* 16 [1916]). Or look at Socrates' words in the *Republic:* "Has it not been said of old that Love is a tyrant? . . . Does not the drunken man have something of the tyrannical mind about him?" (573b [1900]). The tyrant is mad, supposing that he can rule not only over human beings but over gods as well (573c). Freed from all shame, making no distinctions between men and women, beasts and men, women and gods, he does not hesitate to murder any man or sleep with his mother or any god or beast (571cd; cf. *Oedipus* 791 [1928]). In Socrates' stylized history tyranny follows a democracy where citizens see no boundaries between citizen and noncitizen or between human and animal. In the almost surreal democracy of the *Republic,* this lack of distinction takes the form of a gentle acceptance of all people. But when democracy becomes tyranny, the lack of distinction takes the form of indiscriminate violence. Plato's picture of a man who does not distinguish between his mother and other women, between the citizen and the noncitizen, between human and beast is meant to frighten us.

Oedipus' tyranny is not violent. He is

not the Caligula or Nero of the Theban world; he eats no babies, cuts off no heads. Indeed, he is a caring ruler, gentle to his subjects, who turn to him as the savior (48) who will set their polis upright again (46, 51) by ending the plague that is killing the young and as yet unborn of the city. Oedipus shows compassion for them in their suffering and devotes himself to healing the city's sickness. "O pitiful children," he calls them. ". . . I know that you all are sick. . . . My soul groans for the polis and me and you" (58-64).[9] The citizens with their pleas and supplications do not wake him as from sleep, but "weeping" he has "traveled many roads of wandering thoughts" (66-67), calculating how to cure the city. Already he has sent Creon to Delphi to inquire about the source of the pestilence.

The tyrant among the Greeks often sees himself as equal to a god (*isotheos*), freed from the laws that limit other humans (Knox 1957; Vernant 1983, 207). In the first scene of the *Oedipus* the Thebans do appear as supplicants before Oedipus' altars (16), almost as if he were a god. The priest speaks of others in the city who likewise sit in supplication before the altars of Pallas Athene. Yet, despite the supplication and the incense burning, the priest makes clear that Oedipus is not considered a god. "Neither I nor these children here have come as supplicants making you equal to the gods" (31-32). They refer to him as the "first among men" (33) and the "best of mortals" (46), but he remains human. Nor does Oedipus claim divinity. He does not test the gods but respects their speech and their actions.

Oedipus displays none of the negative traits that may be associated with the fifth-century tyrant. He is not like the Alcibiades of Thucydides, whose lawlessness makes the Athenians suspect pretentions to tyranny. And yet the language of tyranny is on almost every page of the play. Knox (1957) in his seminal work, *Oedipus at Thebes*, explains the use of this language with reference to Athens' growing empire during the fifth century—its position as *tyrannos* vis-à-vis the other cities within its orbit (Thucydides 2.63.2, 3.37.2)—and thus reads the play as a warning about the excesses in politics that led to the defeat of Athens in the Peloponnesian War. Such an interpretation captures well the political force of Greek tragedy as a medium for speaking to the Athenians as citizens of a polis, but it flattens the significance of Oedipus' role. Rather, we must see in Oedipus' "tyranny" not the desire to rule over others or the mad rejection of human and divine laws but rather the ambiguous grounding of his political rule and the freedom of his intellect.

Oedipus acquires authority in Thebes by answering the riddle posed by the Sphinx. As he says in anger to Teiresias, "I, coming, know-nothing Oedipus, stopped [the Sphinx] when I hit the mark, knowing with my mind [*gnome*] and not from the birds" (396-98). In the late moments of the play, the chorus sings about the former glory of Oedipus and says, "He, shooting his arrow high, ruled in all respects in happy prosperity, O Zeus, having destroyed the maiden riddler with the crooked nails; within my land he arose, a fortress against death—from which time you were called my king [*basileus*], . . . ruling in Thebes" (1197-1203). The "arrow" that he shot had only the force of his intellect. His strength lies in his mind and his own authority rests on that intellect. As he taunts Teiresias, "Come now, speak, where were you with the prophet's wisdom? Why, when the singing dog [the Sphinx] was here, did you not bring release for the townspeople?" (390-91). And then in a perverse attack on Creon, whom Oedipus suspects of trying to steal away his authority, Oedipus claims that Creon is a fool for not understanding that *tyrannida* is taken with numbers, friends, and a great deal of money (540-42). This, however, is not how Oedipus came to power in the city. He believes that he relied on no one and on nothing except his own mind. Oedipus—who indeed knows nothing about what is most important, the limits set by his own past—sees all as free and open. Thus his ascension to power in Thebes comes as a gift in thanks for his human intellect. His tyranny, as he sees it, is freedom from the past.

As the clues to the mystery of who killed Laius bring Oedipus closer to seeing himself as the object of his own search, it is Jocasta, his mother-wife, who offers an escape from the encroaching evidence against him. At first she appears the female analogue of Oedipus, using argument and calculation, insisting that many cannot be one. The lone witness to the murder of Laius had reported that it was a band of robbers—and the plural is emphasized—who were responsible for the attack.[10] Lone Oedipus could not have been the author of the deed. "Know that this is how he spoke," she says. "It is not possible for him to go back on this. The polis heard these things, not only me" (846–49). But this is only a straw at which Jocasta grasps. In fact, Jocasta views the whole world as chaotic, without divine boundaries or set limits to human behavior; the prophecies of the gods, those orderers of the universe, are worthy of derision: "Why should the human being fear, for whom the things of chance rule. Foresight takes no part in wisdom. It is best to live at random, however one is able" (977–79). Earlier she had told Oedipus that she would look to prophecies "neither now nor henceforth" (857–58). They were wrong about Laius's death; he was not killed by their son—the baby who died on the mountain. The supposed divine order is merely a source of fear for those who do not see clearly.

The chorus is frightened by the impious implications of Jocasta's language and ask that "fate find me bearing holiness and piety in both word and deed—which the high-footed laws prescribe, born in the airy heavens whose only father is Olympus, nor did mortal nature given them birth" (863–70). The chorus retreats to an order that is decreed from above and is unchanging, subject neither to human manipulation nor to control by speech. Jocasta's willingness to cast aside any divine order as she begins to recognize her son in her husband appeals to Oedipus. His arrogant feeling of the power of the human intellect, which had given him political power and had led to his harsh dismissal of Teiresias, is now prepared to challenge the gods as well, but he never goes as far as Jocasta.

After Jocasta has grasped that Oedipus is her son and the murderer of Laius, she is still willing to live with the apparent impiety, to scorn the gods and their divine rules. She tries to keep Oedipus from discovering the truth about his origins, a truth she recognizes before he does: "Do not, by the gods, if you care for your life, seek out these things. . . . I beg you not to do this" (1060–64). Though Jocasta comes to recognize the accuracy of the prophecies of the gods, she continues to reject the divine order. She is the tyrant who will knowingly overstep any restraints, any ancient laws, just as the tyrant of book 9 of the *Republic* and the Alcibiades of Thucydides' Athenians. Oedipus, who had unwittingly overstepped the boundaries that the gods had established, is never so "tyrannical." He tries to be a pious tyrant and fails.

The interchange between Jocasta and Oedipus is one of the highlights of Sophocles' play as the wife-mother begs her husband-son to view the world as random and to ignore the question of his own origins. Oedipus, who understands so much on the abstract level, misconstrues Jocasta's pleas, wrongly concluding that she, with a woman's arrogance, is ashamed that a lowly birth may lie in her husband's past. While he will not accept her urging that the divine world is chaotic, he does revel in the idea of chaos on the human level—a chaos that liberates him from any restraints the past may have placed on him. He ascribes to the human world the disorder Jocasta argues for the divine and calls himself the child of Chance, "she who gives well." He is not ashamed of this: "I was born of this mother, and the months—my brothers—defined me as both small and great" (1080-84).

The aristocratic concern with paternity—and thus legitimacy—whether political or familial, fades before the individual freedom to become great on one's own. A drunken man's taunt that Oedipus did not know who his parents were (779-93) precipitated the trip from Corinth to Delphi and then on to Thebes. While he was still in Corinth the question of origins determined his place in the world. Once away from Corinth, he secures his place in the world on the basis of his mind. Birth and origins no longer matter to him. Thus, suddenly buoyed by this hope that he is the child of Chance, Oedipus sees an opening, a vista of freedom for himself—and indeed for all men.[11] The possibility that he is such a child, born on the mountainside, opens the door to the greatest freedom, the freedom to be anything, to see no limits coming from his birth or physical form. A wild child of the mountains, he could become the king of Thebes. Released from all concern with origins and legitimacy, he is the tyrant who can be anything.

From such mad optimism Oedipus and the chorus will crash into the realization that he is not Chance's child but is bound ever so tightly by the nature of his birth. He is a man of history and of place. When he discovers that he is not a tyrant because he is the legitimate heir to the kingship, he also loses the freedom he would have had as Chance's child. As he discovers his legitimacy, he finds that he has been a tyrant in the more vicious sense, for he has transgressed the most profound of human and divine laws in the murder of his father and the sexual relations with his mother. As Oedipus has his final moments of hope and the chorus sings of the nymphs who may have borne Oedipus, the audience knows that Oedipus is not Chance's child, that the belief in such freedom in this world is ill founded, that all is circumscribed. Oedipus' open vistas close, and he learns that human actions are not free but are limited by the past. When the truth of Oedipus' birth is revealed, the tyrant becomes the king. As the biological legitimacy of his rule surfaces, the tyrant as "unborn," freed from the limits of the physical world, becomes in the tragedy of this play a "born" king. As tyrant Oedipus was a good ruler. Now that he has been revealed as king, he is revealed as the transgressor of ancestral laws, the madman who—in the language of the *Republic*—does not distinguish between his mother and other women, his father and other men.

Reason

The talon-nailed Sphinx sits high on the road outside the city of Thebes shrieking forth her unmusical songs. Oracular in her speech, the winged virgin demands her tribute—the young men of the city—until someone, by solving the enigma she poses for them, can destroy her. None succeed until "know-nothing Oedipus" (397) wanders by in his resolve to escape the fate prophesied by the god Apollo—that he is to kill his father and have sexual intercourse with his mother. Thus, on the road to Thebes, the Sphinx asks who the creature is who walks on four feet in the morning, two in the afternoon, and three in the evening. "Man," answers Oedipus. And from her high cliff the Sphinx throws herself to her death, and Oedipus becomes ruler in Thebes.

Others who had stumbled in trying to resolve the enigma met cruel deaths. What peculiar mental ability enabled Oedipus to succeed where others have failed? As one crippled by the stake that pierced his feet as a baby—and perhaps leaning on a staff—(Benardete 1964, 1), he understands the issues of walking as others do not. He sees the differences between himself and others, they who walk comfortably on two feet in the afternoons of their lives. Specifically, Oedipus can abstract from himself, who walks on three feet in the afternoon. He does not rely on direct experience for any answer, but—freed from the limits of the body—he calculates and discovers the unity of the three metamorphoses to which the Sphinx' enigma referred. Thus he can save Thebes from its suffering even though he has never been taught by the city. His mind has the ability to abstract, he recognizes unity in diversity, and he sees that the several can indeed be one, that one can encompass many. Four-, two-, three-footed animals are one animal: morning, noon, and evening are one life. He sees fluidity in form where others can only see the unique and the particular. Thus he can add and he can count, for measurement entails making the diverse commensurate, abstracting from the particular, seeing the "chair" in the settee and the porch swing or the "man" in the baby and the tottering old fool or the Theban in the old priest and the soft-cheeked boy.[12]

Oedipus is able to move from the particular across boundaries to the general. The relationship between the one and the many does not confound him, he who in his own person expresses an unholy unity. Unlike the others, who fell victim to the stranglehold of the Sphinx, he transcends divisions. Oedipus, however, as one who is marked by the incest in which he unwittingly engages, carries the unity of apparent opposites to such impious extremes that he becomes the pollutor of Thebes rather than its savior. In the words of Teiresias, Oedipus is to his children at once father and brother and to the woman from whom he was born both son and husband and to his father killer and "fellow sower" (*homosporos*) (457–60). Once he has discovered the unity in his own person, he cries out to the chorus, "O marriage, . . . you showed forth fathers, brothers, kindred blood, brides, wives, and mothers" (1403–7). Oedipus' personal life, destroying the boundaries between things that must be kept separate, parallels the transcendence of boundaries that characterizes his intellect.

The tyrant, likewise unifying what is multiple and transcending the boundaries of the traditional laws, threatens the vitality of the city. By freeing himself from the past in terms of origins of political power and ancient laws, the tyrant destroys the community over which he rules. The chorus, in a famous phrase claims, "Arrogance breeds tyranny (*Hybris phuetai tyrannon*)" (873). This passage has perturbed many (Scodel 1982). Lord Acton's adage about power's corrupting has clouded our consciousness and many an editor has worried over how to get this meaning back into the Greek. But Sophocles' chorus means what the words say: *hybris*, arrogance, a sense of freedom from the measures or boundaries that control other human beings, gives birth to the tyrant, he who madly does all, regardless of the laws of men or of gods.

The tyrant is often faulted in ancient Greek authors for treating the city as if it were his family and for failing to recognize boundaries between the private world and the public world of the city. The tyrant uses the wealth of the city as if it were his and the people as if they were his slaves. Oedipus' intellectual style of dissolving boundaries defines as well how he rules in the city. He is not sinister in his confusion of these boundaries between self and city and between city and family, as the Greeks suggested many tyrants were. The mind that broke down the divisions that the Sphinx had posed also breaks down the divisions in the city. He himself becomes the city and the city becomes his own person. While each one of the citizens is sick and each weeps for himself alone, he, Oedipus, as the bodily unity of the many citizens, weeps for all, "for the city, for myself, for you altogether" (64). Creon, who will act as tyrant in the *Antigone* here complains, "There is also a part for me in the city, not for you alone" (630). But it is also Oedipus' identification with the city that pushes him to pursue a cure for the illness of his subjects.

Even more powerful than this identification with the city is the transformation of the city into his family.[13] In the very first words of the play, Oedipus calls to his subjects, "O *tekna*" ("O offspring"). Disregarding the processes of birth and reproduction, Oedipus calls them his offspring not once but twice (1, 6). It is the priest who shifts from *tekna* to the more neutral *paides* (children), a term that Oedipus picks up in the first words of his next speech: "O *paides oikitroi*" ("O pitiful children") (58). Though Oedipus poses as a father to his city, he is father to a city that is sterile. The women are unable to bear children, suffering long in fruitless labor pangs (26–27). The seeds of the crops wither, as do the cattle in the fields. The natural world of Thebes, polluted by the presence of the man who unites in his body what must be separated, rebels against the processes of generation. Thus while the physical world denies women and men their offspring, denies the seeds their sprouts, Oedipus generates through speech and abstraction the *tekna* in the world of the polis. The man who conquers the Sphinx by overcoming the apparent differences between two, three, and four feet fails to acknowledge the difference between the theoretical world of the city created by the human mind and the intimate physical realm of the reproductive family.[14]

Oedipus does not transform his city into his family only through language images. When Creon reports the oracle's demand that the murderer of Laius be expelled from the city, he does not say who is to find and punish the guilty man. Oedipus immediately commits himself to this endeavor,[15] though he later admits the impropriety of this pursuit, since he is a stranger *(xenos)* in Thebes (218, 220, 813, 817). According to Attic law, at least, homicide cases were the responsibility of the kin of the murdered man (Gagarin 1981 and MacDowell 1963). The public prosecutor was unknown. Thus Oedipus acts as a relative of Laius, little understanding the dramatic irony entailed in such a task.

Oedipus expresses surprise that the murder of the king was not investigated at the time of the murder. "With your sovereignty [*tyrannidos*] thus fallen, what evil impeded the search?" (128–29). Obviously, it was the Sphinx. But instead of personalizing the issue—seeing the familial ties of Creon to Laius through Jocasta—Oedipus focuses on the impersonal, on the political possibility of sedition and the abstract notion of "sovereignty,"[16] the abstract unity of the city incorporated in the body of the king. In part because the plague affects the city as a whole, Oedipus takes Laius out of the context of the family and sets him into the abstract world of the polis.

Since the murderer of Laius had not been pursued before, Oedipus himself will pursue him "as if" *(hosperei)* Laius were his own father (264). Despite all the dramatic irony in this image of himself as related to Laius, Oedipus cannot, at least early in the play, call himself kin to the murdered man. He must still introduce the *as if*, the simile that underlines the illegitimacy of the search he undertakes for the city and indicates his denial of the traditional laws concerning the prosecution of evildoers. The simile again shows

Oedipus' capacity within the context of the city to move beyond the particular. The *as if*, while speaking one language to the audience who know the secrets of Oedipus' life, also alerts us to what is not: the city is not a family, and it is only the ability of Oedipus' mind to abstract from the particular that allows him to work on this level of abstraction where the former king of Thebes can be treated "as if" he were Oedipus' father. The images that Oedipus introduces to establish the city as family underscore the inadequacy of this analogy. The citizens of Thebes are not his *tekna*, his offspring. This is not to suggest that kinship is irrelevant for the city. Traditional legitimacy depends on the lines of blood relations. The curse that manifests itself in the lives of the citizens of Thebes reveals the relationship between the purity of the family and the purity of the city. But to treat the city as a family, as Oedipus does, is the mark of tyranny.[17]

Oedipus' search entails all the language of a legal investigation, but through the course of the play that search is transformed from a formal investigation to the most personal exploration, "Who am I?" Oedipus must reorient himself from the abstract city in which he has unified the many and with which he identifies to the most particularized and unique "I." As he says toward the end of the play, "My evils no one else except me among mortals is able to bear" (1414-15). From his identity with the city over which he ruled, he learns of his own uniqueness and the boundaries that separate him out from others.

When Creon returns with the message from Delphi, Creon suggests that Oedipus may wish to hear the oracle within *(eso)* in privacy (92). Oedipus refuses: he will hear the message *es pantes*, before all (93). Creon—dull-witted in this play—sets up boundaries between the many—the city—and its leaders. Oedipus smashes those boundaries. His openness reflects his attempts to obscure the lines that separate the private and the public world, the traditional contrast between the family and the city (Saxonhouse 1983). Indeed, the search will go on within his household as well as without: "If [the pollutor] is at my hearth in my household with my knowledge may I suffer the curse I pronounced on others" (249-51). Oedipus makes no distinction between his own needs and those of the city. Creon recognizes the need to separate the public and the private. At the end of the play, when Oedipus appears on stage, his self-mutilation vividly apparent to all, the private tragedy reveals itself to the city. Creon, maintaining the distinctions between public and private, between leaders and subjects, sends Oedipus away from public view: "As quickly as possible, go into the house [*es oikon*]" (1429, 1515). Creon has a sense of shame, of limits on what we can do in public and what we can reveal of ourselves to others. The openness of Oedipus is shameless; he holds back nothing. It is precisely that openness that enables him to save the city at the same time that he becomes its pollutor.

The clarity and openness of Oedipus' reason contrasts starkly with the hidden knowledge of Teiresias, he who may see things "most clearly" (286). In his early confrontation with the prophet, Oedipus taunts him: Why was the private wisdom of prophecy not effective against the singing dog? (390-92). Teiresias, the medium through whom the gods transmit their laws to men, cannot on his own save the city; and in his frequent exhortations for Oedipus to cease his search, he appears ready to condemn the city to unending suffering. From Oedipus' vantage all knowledge must be open to all. There should be no boundaries between those who know and those who do not know. The message from Delphi is for all Thebans to hear. Words for bringing to light (*phano*) are repeated throughout the play. The plea is to open the unknown, to reveal what is hidden in shadows. With all uncovered, the distinction between public and private knowledge vanishes. Thus Oedipus cannot understand Teiresias' willingness to hold back knowledge and to prevent some from knowing the whole truth. Oedipus sets no limits to what we must know. Teiresias does, for he understands, as Oedipus does not, that unfettered knowledge has its own dangers and that the answer Oedipus seeks is far more complex than Oedipus, whose intellectual skills lie in simplification, realizes.

The tragedy of the *Oedipus*, of course, lies in the fact that Teiresias' aim to hide the knowledge he has received from the gods leaves the sickness in the land, while Oedipus' attempt at discovery reveals that the pursuit of knowledge can uncover pollution at the same time that it saves. Sophocles does not leave us siding with Teiresias, that some things are best shrouded in mysteries. Oedipus is the hero in his testing of limits, in his quest to go beyond. The plague forces him to search. The uncovering must take place, but that uncovering is not without its own tragedy precisely because what is revealed is not freedom, but the *metra*. The revelations of the play uncover the unintended consequences of his intellectual and biological freedom. He must incorporate limits not only on the powers of reason, but also between individual and city, between public and private.

The chorus knows well that there are limits—that there are divine laws that do not grow old and that are not generated by mortal nature but are born with Olympus alone as their father (862–71). They are the laws that Antigone in her tragedy will describe as "unwritten" (454) and that today we might refer to as "natural laws." Within this realm Teiresias works, revealing the laws of the gods to the city. He knows structure and understands limits. For Oedipus all is fluid, as man, for example, moves from one form to another, from one period in life to another or as the city becomes the family and the family becomes the city.

In contrast to Oedipus' equation early in the play between the city and family are the powerful final scenes in which Oedipus weeps for his daughters, his own half-sisters. His intellect had enabled him to succeed in the world of abstractions and in the political world where the many must, in some fashion, become the one in order for the city to exist. At the end of the play, his attention turns to the particular, to his own offspring rather than the citizens whom he called *tekna* in the first line of the play. From the concern with the sterility of the city he turns to the sterility of the family. No one will marry his daughters, he laments, polluted as they are by their double relationship to him. Unlike the city and unlike the earlier understanding of his relationship to Laius, they are bound to him not by the *as if*, not by simile, but through the biological processes of reproduction. The intellect of Oedipus' earlier life created abstractions from the particular—the children of the city from the particular child. Now he returns from abstraction to particularity.[18]

As Oedipus pursues his quarry himself, he discovers that reason is not enough to rule. Teiresias fears Oedipus' reliance on reason, knowing that the very reason that saved the city the first time and put Oedipus in power may save the city again but destroy Oedipus. While reason can destroy the Sphinx, it also enables Oedipus to learn of the impieties that lie at his birth and at the foundation of his rule. Despite Oedipus' early focus on the world of abstraction, a concern with the particular facts about his own birth motivate him. Thus Teiresias, insulted by Oedipus, upsets Oedipus in turn by asking, "Do you know from whom you are?" (415). Coursing powerfully through Oedipus' psyche was his curiosity and his fear about his birth. A drunken man's taunting him with the suggestion that he was not his father's son upset him deeply (781). Even after assurances from his presumed parents, Polybus and Merope, he let the worry get under his skin (786), headed towards Delphi and the oracle's response that set him on the path to where the three roads meet, and then went on to where the Sphinx reigned.

Oedipus in his search for his own origins has raised the problem of the importance of history. As the "know-nothing Oedipus," as "untaught" Oedipus, he can measure (795, 1110)—see the unity between the one and the many—but he does not understand the past as a limit on what one can achieve or know. He tries to cast off his history by coming to Thebes. Sophocles dismisses such optimism, for such actions entail, as well, the dismissal of the gods and their prophecies. Had Oedipus accepted the god's words, he would have returned to Corinth, killed Polybus, married Merope, and thereby avoided the fate predicted for him. Although along with the chorus he was shocked by Jocasta's dismissal of divine order, he himself, by trying to escape his origins and the future foretold for him, demonstrated the same impiety.

Oedipus searches for a freedom from the past, but it is a freedom that the poet and the gods do not allow him. It is this struggle between freedom and openness of the mind and the limits of biology and history that lies at the heart of the tragedy of Oedipus. He who is free to answer the riddle of the Sphinx by going beyond the particular must learn the importance of the particular. In the poignant final scenes Oedipus has, with his blindness, turned to his particular children rather than the abstract children of the city. He calls to them, laments for them. Still he emphasizes a unity with his daughters, but the focus has shifted from the general to the particular, from the abstract to the precise. He longs for the unity afforded in the private world: "Touching them with my hands, I might seem to have them" (1469–70).[19] Such unity and identification with the city, with the public world, has been denied him.

The movement of the play is in part the discovery of the importance of boundaries on what we, as human beings, can accomplish. This education takes us from the unity of city—one in which youth and age are jumbled together and the ruler is identified corporeally with the city—to one in which Oedipus weeps for himself and his daughters.[20] He who could cut through categories learns that differences do exist between the many and the one, the mother and the wife, the daughter and the sister, family and polis, ruler and his city. Reason and tyranny play the same role in the world of the polis. In one sense they can liberate and save the city from its suffering, can bind the city together into a unified whole; but in another sense, by obliterating differences they bring on the wrath of the gods. Oedipus, as the tyrant and the paradigm (*paradeigma*, 1193) of the rational individual, learns vividly that the reason that can unify cannot alone be the basis of his rule: his birth—his bonds with the physical world—are the origins of his power. Before, he relied on nothing but the brilliance of his mind; "know-nothing" and "untaught" were the phrases he used in self-descriptive scorn of those who he thought threatened him. At the end, in his blindness, he has learned that knowledge is more than calculation and that political power is not only the gift of reason.

Limits (*Metra*)

Greek tragedy and Greek thought in general explore the tension between limits and freedom of action. Heraclitus in his cosmology saw limits that constrained even the movements of the divine Sun. The orderly world will not brook independent actions even on the part of the gods. Herodotus gave limits a physical form in the geographical boundaries over which kings marched to their own destruction. Herodotus called the enforcement of these limits not the justice of the Furies, as Heraclitus had, but the jealousy (*phthonos*) of the gods; this jealousy preserved an order among men, allowing no man in his pride to become too great. In Thucydides the limits do not have the visual impact of rivers and mountains but appear rather in the consequences of the dynamic of the growth of cities. The Athenian's limitless desire for more (*pleonexia*) sends Athens on the ill-fated expedition to Sicily. Oedipus is like all these characters from the writings of the Greeks. His ability to reason and to ignore traditional boundaries in his mind and in his body makes him like the errant Sun; like Cyrus who, in Herodotus' tales, crosses the Araxes; like the Athenians who send troops to Sicily. Sophocles tells the audience about the tragic limits to human knowledge and to human power. Boundaries hold back the Sun; the same boundaries restrain the tyrant, the measurer, the calculator.

The *Oedipus* in its cathartic final moments, as the self-blinded tyrant-king gropes for his children, demands that the audience fear the awesome power of abstraction and the freedom of the ruler's intellect. The tyrant as the "unborn" and the tyranny of reason bring tragedy to the family and the city. In his capacity to abstract, he has lost a sense of the limits that his own origins set on himself as well as on the city. The tragedy of the *Oedipus* draws for us both the glory and the failure of the individual attempt of the political actor to rise above the mere body and build a world where reason, released from the defective body, alone is power.

Sophocles offers his play as a warning. Humans—tyrants or not—attempting the transformation of the world on the basis of abstract, calculating reason alone, with no regard to the limits of history or of piety, will call forth the Furies enforcing the *metra*. Sophocles urges the audience of Athenian citizens to reflect on the consequences of viewing the world as a sea of undifferentiated unity, where the child is the youth is the grandfather in the comprehensive concept, "man," where "the Theban" is at the same time the "young not yet ready to fly" (16–17) and the old priest. Such abstraction belongs to the divine world of "unborn laws," towards which the city in its own desire for universality strives, while the individual, no matter how wise, must acknowledge differentiation and the boundaries between self and the ruler in the city, between city and family.[21]

Oedipus stands forth as a "paradigm" (1193), a shocking example to the chorus and to us of the excessive unity and overstepping of boundaries that brings sterility to the city. The city itself flourishes and gains life from differentiation. The son cannot also be the father. The city cannot also be the family. Yet, to exist, the city must rise above the particular and unify the many, see "the Theban" in the young boy and in the old priest. The balance is difficult and Oedipus in his person and his intellect captures the excesses of such unity. The tragic vision of Oedipus shattered by the knowledge of what he has done does not resolve the tension or delimit where the boundaries are or how we can recognize them. On the one hand we must abstract and generalize, move from the particular to the universal and not be hedged in by the particular. On the other hand this drive can be dangerous if we lose sight of the particular and unify what cannot be unified, the mother and the wife, the sister and the daughter, the family and the city—as Socrates, for example, argues in the fifth book of the *Republic*.

In some sense we are all like Oedipus, not in Freud's terms of our psychosexual development, but rather in our desire to theorize, to impose an order on the world in which we live, while rejecting the randomness of Jocasta's world or the privacy of Teiresias. As theorists, we see the world encompassed by human intellect and we value the coherence that Oedipus wishes to impose on the world. We can also see in this theorizing impulse of Oedipus that tyranny does not necessarily come from what is base or ignoble but from what is perhaps most noble and what is most daring, what Oedipus and Socrates in a sense share—in their transcendence of tradition, in the denial of limits and of origins, and in their care for the whole. At the same time however, Oedipus the tyrant, when he appears before us on the stage, bloodied, groping for his daughters, incorporates the tension between the limits we must observe and the freedom of our intellect. He becomes the tragic symbol of the tyranny of reason in the polis.

Notes

Earlier versions of this paper were presented at the 1987 annual meeting of the American Political Science Association, Chicago and as the Arthur M. Wilson Memorial Lecture at Dartmouth College, 1988. I have benefited greatly from comments on both those occasions.

1. Strauss, in response to events of the twentieth century and to a political science that he saw as "haunted by the belief that 'value judgments' are inadmissible in scientific considerations" tried to revive *tyranny* as a term to describe "a danger coeval with political life" (1963, 21–22) but his attempt met with little success at least in the "mainstream" professional discourse. While there is some overlap between our understandings of the meaning of tyranny, I want to draw out the positive aspects of tyranny as well as the negative ones so as to portray the ambiguous status of the term.

2. The language here is meant to recall the language used by the Corinthians to describe the Athenians in Thucydides (1.70.2). See also Callicles' description of the "real man" in Plato's *Gorgias* (491b [1900]).

3. Fragment 44 in Kahn 1979 (p. 48).

4. Benardete (1964, 2) sees a movement "from a political to a family crime." He argues that the latter is the "more comprehensive."

5. The literature on the *Oedipus* is so thick, the insights so varied that one cannot take account of all the interpretations and analyses that have emerged in the last decades. Much of the literature on the *Oedipus,* however, is concerned with the relationship between Oedipus and the fates, whether Oedipus because of his anger or his suspicions of others in some way "deserved" to suffer as he did. After all, his parents were the ones who tested the oracle and the shepherd who kept him alive disobeyed orders from his ruler. This tack would take us into the question of the justice of the Greek gods. I wish here rather to plumb the play for what it tells us about reason, power, and politics. Little of what has been written has dealt with the political thought implicit in the *Oedipus.* Those works that do treat the play with a view to its political thought that I have found most useful include Benardete 1964, Schwartz 1986, Segal 1981, and Zeitlin 1986.

6. The Greek word *oida* means both "know" and "see"; the word *oideo* means "swell." Sophocles takes full advantage of the verbal games such language allows him. See, e.g., lines 16 and 292.

7. In many ways this play can serve as a prelude to Plato's epistemology. In particular, the image of divided line in book 6 and the cave in book 7 of the *Republic* tie these two works together. Cf. Nussbaum (1986, 13) who suggests that Plato must be read in the context of the poetic tradition: "Plato's writing so continually alludes to his poetic context in its choice of image, story, and turn of phrase that the meaning of many salient details is lost on us if we do not try to approach him in awareness of this context." See nn. 12 and 17. Interpretations of this tragedy have often focused on Oedipus' "love of learning" as his "tragic flaw." But within the whole play "love of learning" cannot be dissociated from the political context. Oedipus does not want to know who killed Laius for the sake of knowing; he needs that knowledge because he is the ruler of Thebes. Similarly, the knowledge he *can* gain depends on who he is.

8. Some unknown Hellenistic editor is responsible for the title *Oedipus Tyrannos.* So far as we know, Sophocles titled his tragedy simply *Oedipus.* Yet, the epithet *tyrannos* (unlike the Latin *rex)* is apt. Andrewes notes that a tyrant "in these Greek terms, is not necessarily a wicked ruler, but he is an autocrat (and generally a usurper) who provides a strong executive" (1956, 7) and that "tyrant, which was never any man's title, is the appropriate word for a man who seized power in an irregular way" (p. 28).

9. All translations are my own. The elegance of the poetry is sacrificed for the sake of precision.

10. The curiosity is that Oedipus responds in the singular, changing the number of robbers from many to one (122–24, 292). Vellacott (1971) uses this change as the basis for his novel interpretation that Oedipus knew all along that he was the murderer of his father and husband of his mother.

11. Segal reads this passage somewhat differently: "He lacks the basic information about his origins that gives man his human identity and sets

him apart from the undifferentiated realm of nature and the anonymous realm of beasts" (1981, 207). I read a more optimistic tone into the passage than Segal, sensing on my part the antiaristocratic implications of the words. I find myself more in sympathy with Bowra's remark, "He is a self-made man who will be remembered not as the son of someone, but as the man who answered the Sphinx's riddle and became king of Thebes" (1944, 198).

12. Socrates' discussion of the divided line in the *Republic* (509d–11e) is apposite here. Benardete comments, "Oedipus saw the heterogeneity that underlies an artificial homogeneity" (1964, 6). I am not sure why Benardete calls this "artificial," unless it is because the homogeneity is imposed by the human intellect. Knox draws out well the use of numbers throughout the play and Oedipus' role as the measurer, the one who proceeds through calculation (1979, 101).

13. In contrast to Benardete's emphasis on the focus away from the political to the family (1964, 3, 9), I see the development in terms of the separation of the two that Oedipus must acknowledge to understand his tyranny. Benardete is not entirely consistent here, for while he sees a "shift," he also argues forcefully that Oedipus violates "equally the public and private with a single crime."

14. This dichotomy between family and city obviously ignores many of the overlapping points between the two and their interdependence. In Greek literature, however, this dichotomy plays a frequent and profound role. For a fuller discussion of this opposition see Saxonhouse 1980 and 1983.

15. Here again we must note the similarity between Oedipus and the Platonic Socrates. The oracle as reported by Chaerephon gives no command to Socrates, though Socrates interprets it as one (*Apology* 23ab [1900]).

16. I realize that "sovereignty" is not a very good translation for *tyrannis, -idos* but I am hard pressed to find the English equivalent that can capture the abstraction of the word and not also transfer to it the negative connotation of "tyranny."

17. The connection with Plato's *Republic* is important. Consider in this context the city in book 5; see further Nichols 1987, chap. 3.

18. See n. 4.

19. The impious sexual overtones of this sentiment remind us that the boundaries must exist within the family as well as within the city.

20. The failure of Oedipus may perhaps be matched by the success of Sophocles, who as tragic poet can combine the knowledge of Teiresias with the reason of Oedipus in one play. On the peculiarly privileged position of the playwright in Athens see further Euben 1986, chap. 1.

21. See further Saxonhouse 1988.

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LIBERAL VIRTUES

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I explore possible links between liberalism and conceptions of individual virtue. I first distinguish between virtue seen as instrumental to the preservation of societies and institutions and virtue seen as intrinsically valuable, that is, as an end in itself. I argue that certain distinctive instrumental virtues are required for well-functioning liberal societies, economies, and political institutions. I then sketch different versions of liberal intrinsic virtue and explore the tensions among them and between them and liberal instrumental virtue. I conclude with some competing conjectures as to what extent necessary liberal virtues are being fostered through current institutions.

For two generations, scholarly inquiry has been dominated by the belief that the liberal polity does not rest on individual virtue. On the theoretical plane, liberalism has been understood as the articles of a peace treaty among individuals with diverse conceptions of the good but common interests in preservation and prosperity. On the level of basic institutions, the liberal constitution has been understood as an artful contrivance of countervailing powers and counterbalancing passions. In the arena of liberal society, individual behavior has been analyzed through the prism, and public policy guided by the precepts, of neoclassical economics.

Virtues versus Interests in Contemporary Liberalism

The conclusion that liberalism could be severed in theory and in practice from the concern for virtue was shared by scholars of widely divergent orientations. While Leo Strauss was on the whole sympathetic, and C. B. Macpherson hostile, to the liberal polity, they converged on an interpretation of Locke that stressed his effort to liberate individual acquisitiveness from traditional moral constraints (Macpherson 1962; Strauss 1953). Martin Diamond and Gordon Wood could agree on the essentials of the interest-based "new science of politics" that displaced civic republicanism and undergirded the Constitution (Diamond 1959; Wood 1969). The understanding of modern liberal society as an agglomeration of self-interested individuals and groups formed a common point of departure for both defenders of pluralism (such as Robert Dahl in his 1950s incarnation) and critics, led by Theodore Lowi (Dahl 1956; Lowi 1969).

Although the various analysts of liberalism were not in agreement on a specific conception of virtue, they united in the accusation that liberal theory and practice were bereft of—if not hostile to—*any* conception of virtue. For Strauss and many of his followers, liberalism placed in jeopardy both the restraints on passion that should govern the daily life of the many and the striving for excellence that should guide the activities of the few. For J. G. A. Pocock and his allies liberalism represented the evisceration of republican

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virtue understood as the disposition to subordinate personal interests to the common good (Pocock 1975; Skinner 1984). For Charles Taylor and his fellow communitarians, liberalism undercut the very possibility of community and thus the significance of the virtues understood as the habits needed to sustain a common life (MacIntyre 1981, 1984; Sandel 1982, 1984; Taylor 1979). To the extent that virtue could nonetheless be found in the actual practices of liberal society, it was to be understood, argued Irving Kristol and many others, as the residue of an older moral and religious tradition at odds with—and under relentless assault by—liberalism's most fundamental tendencies (Kristol 1980).

The proposition that liberalism does not rest on virtue is not the arbitrary invention of contemporary scholarship. Albert Hirschman (1977) has traced the emergence in seventeenth- and eighteenth-century social thought of the thesis that republican government could best be secured not through civic virtue but rather through the liberation of the commercial-acquisitive "interests" of the middle class in opposition to the politically destructive "passions" of the aristocracy. The most famous of the *Federalist Papers* (10 and 51) contain memorable formulations of the need to counteract interest with interest and passion with passion. Immanuel Kant, who was at once the most profound moral philosopher and the most devoted liberal theorist of his age, argued vigorously for the disjunction between individual virtue and republican government:

> The republican constitution is the only one entirely fitting to the rights of man. But it is the most difficult to establish and even harder to preserve, so that many say a republic would have to be a nation of angels, because men with their selfish inclinations are not capable of a constitution of such sublime form. But [this is an error: republican government] is only a question of a good organization of the state, whereby the powers of each selfish inclination are so arranged in opposition that one moderates or destroys the ruinous effect of the other. The consequence . . . is the same as if none of them existed, and man is forced to be a good citizen even if not a morally good person. The problem of organizing a state, however hard it may seem, can be solved even for a race of devils, if only they are intelligent. (1963, 111–12)

In spite of the considerable evidence for the proposition that the liberal-republican polity requires no more than the proper configuration of rational self-interest, this orthodoxy has in recent years come under attack from scholars who argue that liberal theory, institutions, and society embody—and depend upon—individual virtue. Judith Shklar has traced the emergence of liberalism to a revulsion against the cruelty of religious wars, that is, to a decision to replace military and moral repression with a "self-restraining tolerance" that is "morally more demanding than repression" (1984, 5). Rogers Smith has found in Locke a core conception of "rational liberty" on which are based a distinction between liberty and license and an account of individual excellence (Smith 1985). Summarizing a painstaking reexamination of the neglected *Thoughts Concerning Education*, Nathan Tarcov concludes that "instead of a narrowly calculating selfishness, Locke teaches a set of moral virtues that make men able to respect themselves and be useful to one another both in private and in public life" (1983, 137; see also Tarcov 1984). Ronald Terchek extends this thesis to Adam Smith and John Stuart Mill, whom he interprets as recommending "the cultivation of those habits which turned us toward the practice of virtue" (1986, 18). J. Budziszewski offers a general argument for the proposition that liberalism and the cultivation of the virtues can be logically compatible and even mutually supportive (1986). In Harvey Mansfield, Jr.'s striking rereading of the *Federalist*, the "automatic or mechanical" view of our constitutional arrangements is replaced by a focus on well-ordered souls as the foundation of sustainable republican government: "Not only are the people expected to be virtuous but also those who run for office" (1987, 59). James Q. Wilson's survey of contemporary public policy dilemmas concludes that economic diagnoses and prescriptions, which treat individual dispositions as fixed and exogenously determined "tastes," are at best one-sided. The challenge of social policy is not just the manipulation of incentives, but also the formation of character: "In almost every area of important public concern, we are seeking to induce persons to act virtuously. . . . In the long run, the public interest depends on private virtue" (1985, 15–16).

If this line of argument is correct, there is a tension at the heart of liberalism. The liberal state must by definition be broadly tolerant, yet it cannot be wholly indifferent to the character of its citizens. As Thomas Spragens has noted, "a citizenry without public spirit, without self-restraint, and without intelligence accords ill with the demands of effective self-governance" (1986, 43). To quote Judith Shklar once more, the alternative before us "is not one between classical virtue and liberal self-indulgence. . . . Far from being an amoral free-for-all, liberalism is, in fact, extremely difficult and constraining, far too much so for those who cannot endure . . . the risks of freedom. The habits of freedom are developed, moreover, both in private and in public, and a liberal character can readily be imagined" (1984, 5). The challenge, then, is to give an account of individual virtue that supports rather than undermines liberal institutions and the capacious tolerance that gives liberal society its special attraction.

The thesis that liberalism rests in some measure on virtue is not the palpable absurdity that the liberal polity requires an impeccably virtuous citizenry, a "nation of angels." Nor is it incompatible with the mechanical-institutional interpretation of liberalism, for clearly the artful arrangement of "auxiliary precautions" can go some distance toward compensating for the "defect of better motives." Nor, finally, does this thesis maintain that the liberal polity should be understood as a tutelary community dedicated to the inculcation of individual virtue or excellence. The claim is more modest: that the operation of liberal institutions is affected in important ways by the character of citizens (and leaders) and that at some point the attenuation of individual virtue will create pathologies with which liberal political contrivances, however technically perfect their design, simply cannot cope. To an extent difficult to measure but impossible to ignore, the viability of liberal society depends on its ability to engender a virtuous citizenry.

While this requirement is not unique to liberal societies, it poses special difficulties for them. The liberal way of life frees individuals from traditional restraints and allows them to pursue their own conceptions of happiness. To the extent that the liberal virtues are not simply consistent with individual self-interest, processes of forming and maintaining them will come into conflict with other powerful tendencies in liberal life. The liberal virtues are the traits of character liberalism needs, not necessarily the ones it has. Yet it is unnecessary to import these virtues from the outside, for they are immanent in liberal practice and theory. The tension between virtue and self-interest is a tension within liberalism, not between liberalism and other traditions.

The Virtues As Ends and As Means

The classical conception of the relation between virtue and politics was spelled out by Aristotle. Individual virtue (or excellence—the Greek *arete* will bear both meanings) is knowable through everyday experience, definable through philosophic inquiry, and is always and everywhere the same. For Aristotle, the virtues are not just Greek, but rather human, virtues. Political life must be seen as in large measure a means to the attainment of virtue, understood as an end in itself. Once the threshold conditions of physical and material security are met, the political community should structure its institutions and policies to promote virtue in its citizens; and its worth as a community is contingent on the extent to which it achieves that goal.

Aristotle was under no illusion that the communities of his day were actually organized in pursuit of virtue. Some, like Sparta, were devoted to military victory, others to commercial prosperity; most had no single discernible goal. Each nonetheless had a largely tacit, operative conception of the virtuous individual as the good citizen whose character and conduct were most conducive to the preservation of the community and of its way of life. In this understanding, the relation of politics and virtue are reversed: virtue becomes the means, and the political community provides the end.

This reversal gave rise to the question explored by Aristotle in book 3 of the *Politics* (1984): Are the virtues of the good human being and of the good citizen identical or different? It turns out that they are nearly always different, a conclusion that generates a double dilemma. If a community is notably imperfect, the citizen who shapes himself in its image and devotes himself to its service will undergo a kind of moral deformation. Conversely, the virtues of the good human being simpliciter may not only not promote but may actually impede the activities of that person's particular community.

Liberal theorists were not unaware of this dilemma, and they responded to it in two very different ways. Some, such as John Stuart Mill, retained a place for the Aristotelian conception of virtue as an intrinsic good but argued that the practice of virtue, so understood, would also be supportive of the liberal polity. In a liberal order, the same virtues are both ends and means: the good citizen and the good human being are identical.

The other liberal strategy was to cut the knot by denying the very existence of intrinsic virtue, that is, by reinterpreting virtue as purely instrumental to the nonmoral goods that constitute the true ends of liberal politics. Thus Hobbes: "All men agree on this, that peace is good, and therefore also [that] the way, or means, of peace, which . . . are justice, gratitude, modesty, equity, mercy, and the rest of the laws of nature, are good. . . . But the writers of moral philosophy, though they acknowledge the same virtues and vices, [do not see] wherein consisted their goodness; nor that they come to be praised as the means of peaceable, sociable, and comfortable living" (1962, 124).

During the past two decades, John Rawls has moved between the Hobbesian and Millian strategies. For Rawls, the ultimate justification—and overriding objective—of the liberal polity is the attainment of justice viewed not as an individual virtue but as a social state of affairs. Rawls rejects "perfectionism"—the thesis that society should be so arranged as to maximize the achievement of individual virtue or excellence. Yet justice as a virtue predicated of individuals does occupy a place within his overall theory. Individuals are presumed to have a capacity for a sense of justice, that is, the ability to accept and to act upon the agreed-on principles of social justice, which in turn supply the substantive content of individual justice. As this conception is developed in *A Theory of Justice*, the engendering of just individuals is not the goal of liberal society but rather the means to the preservation of that society. That is why each member of a well-ordered liberal society wants the others to have a developed sense of justice. And more broadly, Rawls declares in a Hobbesian spirit, "a good person has the features of moral character that it is rational [i.e., instrumentally rational] for members of a well-ordered society to want in their associates" (1971, 436–37). In his more recent work, however, he has placed increased emphasis on the development and exercise of "moral personality" as an intrinsic good or end in itself. The very practices that help sustain a just society also express our nature as free and equal rational beings who have realized their innate capacity for justice (Rawls 1980; for an analysis, see Galston 1982b).

In the liberal tradition, then, we find traces of both sides of the Aristotelian conception: virtue as end, and virtue as means. It would be surprising, however, if on closer inspection the liberal canon of the virtues turned out to mirror the classical enumeration. Indeed I shall argue (pace Hobbes) that it does not. The liberal virtues are not simply the classical virtues justified on a different basis. They are in important respects different virtues.

Instrumental Liberal Virtues

I begin by examining the liberal virtues understood instrumentally as means to the preservation of liberal societies and institutions. To fix terms, let me characterize the liberal polity as a community possessing to a high degree the following features: popular-constitutional government; a diverse society with a wide range of individual opportunities and choices; a predominantly market economy; and a substantial, strongly protected sphere of privacy and individual rights. And to avoid misunderstanding, let me briefly characterize the status of the propositions I am about to advance.

The discussion of the instrumental virtues is a catalog not of logical entailments within liberal theory but rather of empirical hypotheses concerning the relation between social institutions and individual character. I offer at most fragmentary evidence in support of these hypotheses, an adequate test of which would require a far more systematic historical and comparative inquiry.

When I speak of certain virtues as instrumental to the preservation of liberal communities, I do not mean that every citizen must possess these virtues, but rather that most citizens must. The broad hypothesis is that as the proportion of nonvirtuous citizens increases significantly, the ability of liberal societies to function successfully will progressively diminish.

The fact (if it is a fact) that the instrumental virtues are socially functional does not mean that they are individually advantageous. To be sure there is some overlap between these two objectives. The liberal virtues demand less self-discipline and sacrifice than do the virtues of classical antiquity, of civic republicanism, or of Christianity, and the practice of many of these social virtues will simultaneously make it easier for individuals to succeed within liberal communities. Still, these virtues are not reducible to self-interest, even self-interest "rightly understood." Thus while the liberal virtues do not presuppose a specific moral psychology, they do at least imply the rejection of any comprehensive egoism.

General Virtues

Some of the virtues needed to sustain the liberal state are requisites of every political community. From time to time, each community must call upon its members to risk their lives in its defense. Courage—the willingness to fight and even die on behalf of one's country—is thus very widely honored even though there may be occasions on which the refusal to fight is fully justified.

In addition, every community creates a complex structure of laws and regulations in the expectation that they will be accepted as legitimate—hence binding—

without recourse to direct threats or sanctions. The net social value of a law is equal to the social benefits it engenders minus the social costs of enforcing it. As the individual propensity to obey the law diminishes, so does a society's ability to pursue collective goals through the law. Law-abidingness is therefore a core social virtue, in liberal communities and elsewhere. (This does not mean that disobedience is never justified, but only that a heavy burden of proof must be discharged by those who propose to violate the law.)

Finally, every society is constituted by certain core principles and sustained by its members' active belief in them. Conversely, every society is weakened by the diminution of its members' belief in its legitimacy. Loyalty—the developed capacity to understand, to accept, and to act on the core principles of one's society—is thus a fundamental virtue. And it is particularly important in liberal communities, which tend to be organized around abstract principles rather than shared ethnicity, nationality, or history.

Beyond the virtues needed to sustain all political communities are virtues specific to liberal communities—those required by the liberal spheres of society, economy, and polity.

Virtues of Liberal Society

A liberal society is characterized by two key features: individualism and diversity. To individualism corresponds the liberal virtue of independence—the disposition to care for and take responsibility for oneself and to avoid becoming needlessly dependent on others. Human beings are not born independent, nor do they attain independence through biological maturation alone. A growing body of evidence suggests that in a liberal society the family is the critical arena in which independence and a host of other virtues must be engendered. The weakening of families is thus fraught with danger for liberal societies. In turn, strong families rest on specific virtues. Without fidelity, stable families cannot be maintained. Without a concern for children that extends well beyond the narrow boundaries of adult self-regard, parents will not effectively discharge their responsibility to help form secure, self-reliant young people. In short, the personal independence required for liberal social life rests on self-restraint and self-transcendence, the virtues of family solidarity.

I turn now from individualism to diversity, the second defining feature of liberal society. The maintenance of social diversity requires the virtue of tolerance. This virtue is widely thought to rest on the relativistic belief that every personal choice, every "life-plan," is equally good, hence beyond rational scrutiny and criticism. Nothing could be further from the truth. Tolerance is fully compatible with the proposition that some ways of life can be known to be superior to others. It rests, rather, on the conviction that the pursuit of the better course should be (and in many cases can only be) the consequence of education or persuasion rather than coercion. Indeed, tolerance may be defined as the ability to make this conviction effective as a maxim of personal conduct.

Virtues of the Liberal Economy

The liberal, market economy relies on two kinds of virtues—those required by different economic roles, and those required by liberal economic life taken as a whole. In a modern market economy, the basic roles are the entrepreneur and the organization employee. The entrepreneurial virtues form a familiar litany: imagination, initiative, drive, determination. The organizational virtues are very different from (and in some respects the reverse of) the entrepreneurial. They include such traits as punctuality, reliability, civility toward coworkers, and a willingness to work within established frameworks and tasks. As economic units evolve, one of the great management challenges is to adjust the mix of entrepreneurial and organizational practices. Sometimes this takes the form of an organizational displacement (or routinization) of entrepreneurial charisma, as in the ouster of Steven Jobs as head of Apple Computer. Sometimes it requires just the opposite, as when a large, stodgy organization replaces a centralized structure with semiautonomous units and loosens individual task and role definitions in an effort to encourage more entrepreneurial practices on the part of its employees.

There are three generic (as distinct from role-specific) virtues required by modern market economies. The first is the work ethic, which combines the sense of obligation to support personal independence through gainful effort with the determination to do one's job thoroughly and well. The second is the achievement of a mean between ascetic self-denial and untrammeled self-indulgence—call it a capacity for moderate delay of gratification; for while market economies rely on the liberation and multiplication of consumer desires, they cannot prosper in the long run without a certain level of saving, which rests on the ability to subordinate immediate gratification to longer-run self-interest.

The third generic economic virtue is adaptability. Modern market economies are characterized by rapid, sweeping changes that reconfigure organizations and occupations. Patterns of lifelong employment within a single task or organization—common for much of this century—are being displaced. Most individuals will change jobs several times during their working lives, moving into new occupations, new organizations, and even new sectors of the economy. To be sure, collective political action can help regulate the pace of change, ameliorate its consequences, and share its costs. Still, domestic and international pressures combine to make the fact and basic direction of economic change irresistable. Thus the disposition to accept new tasks as challenges rather than threats and the ability to avoid defining personal identity and worth in reference to specific, fixed occupations are essential attributes of individuals and economies able to cope successfully with the demands of change.

Virtues of Liberal Politics

I come, finally, to the sphere of politics, which calls for virtues of both citizens and leaders.

Virtues of citizenship. Some generic citizen virtues have already been identified: courage, law-abidingness, loyalty. In addition to these are the citizen virtues specific to the liberal polity. Because a liberal order rests on individual rights, the liberal citizen must have the capacity to discern, and the restraint to respect, the rights of others. (Invasion of the rights of others is the form of *pleonexia* specific to liberal political life.) Because liberalism incorporates representative government, the liberal citizen must have the capacity to discern the talent and character of candidates vying for office, and to evaluate the performance of individuals who have attained office. Liberalism also envisions popular government responsive to the demands of its citizens. The greatest vices of popular governments are the propensity to gratify short-term desires at the expense of long-term interests and the inability to act on unpleasant truths about what must be done. To check these vices, liberal citizens must be moderate in their demands and self-disciplined enough to accept painful measures when they are necessary. From this standpoint, the willingness of liberal citizens to demand no more public services than their country can afford and to pay for all the benefits they demand is not just a technical economic issue but a moral issue as well. Consistently unbalanced budgets—the systematic displacement of social costs to future generations—are signs of a citizenry unwilling to moderate its desires or to discharge its duties.

The liberal citizen is not the same as the civic-republican citizen. In a liberal polity there is no duty to participate actively in politics, no requirement to place the public above the private and to subordinate personal interest to the common good systematically, no commitment to accept collective determination of personal choices. But neither is liberal citizenship simply the pursuit of self-interest, individually or in factional collusion with others of like mind. Liberal citizenship has its own distinctive restraints—virtues that circumscribe and check, without wholly nullifying, the promptings of self-aggrandizement.

Virtues of leadership. The need for virtue and excellence in political leaders is perhaps more immediately evident than is the corresponding requirement in the case of citizens. The founders saw popular elections as the vehicle for discerning and selecting good leaders. Thomas Jefferson spoke for them when he wrote to John Adams that

> there is a natural aristocracy among men. The grounds of this are virtue and talents. . . . The natural aristocracy I consider as the most precious gift of nature, for the instruction, trusts, and government of society. . . . May we not even say, that that form of government is the best, which provides the most effectively for a pure selection of these natural *aristoi* into the offices of government? . . . I think the best remedy is exactly that provided by all our constitutions, to leave to the citizens the free election and separation of the *aristoi* from the *pseudo-aristoi.* (Mason 1965, 385)

The leadership virtues specific to liberal polities include patience—the ability to accept, and work within, the constraints on action imposed by social diversity and constitutional institutions. Second, the liberal leader must have the capacity to forge a sense of common purpose against the centrifugal tendencies of an individualistic and fragmented society. Third, the liberal leader must be able to resist the temptation to earn popularity by pandering to immoderate public demands. Against desire the liberal leader must counterpose restraints; against the fantasy of the free lunch he or she must insist on the reality of the hard choice; against the lure of the immediate he or she must insist on the requirements of the long term. Finally, while the liberal leader derives authority from popular consent, he or she cannot derive policy from public opinion. Rather, the leader must have the capacity to narrow—so far as public opinion permits—the gap between popular preference and wise action. The liberal leader who disregards public sentiment will quickly come to grief, but so will the leader who simply takes that sentiment as the polestar of public policy. Through persuasion, the liberal leader tries to move the citizenry toward sound views. But the limits of persuasion must constitute the boundaries of public action, or else leadership becomes usurpation.

As the authors of the *Federalist* insisted, and as experience confirms, there are also specific virtues required for the successful conduct of the different offices in a liberal-constitutional order: optimism and energy in the executive, deliberative excellence and civility in the legislator, impartiality and interpretive skill in the judge (for a parallel account, see Mansfield 1987, 60). And, as Jefferson suggested, the ultimate test of systems of election or appointment is their tendency to select officeholders with the appropriate virtues. For that reason, it is appropriate and necessary to inquire whether particular systems of selection (e.g., presidential nominating primaries) tend on balance to reward the kinds of personal traits that their corresponding offices require.

General political virtues. There are two other political virtues required of liberal citizens and leaders alike. While not all public policies need be made in the full light of day, liberal politics rests on a presumption of publicity, that is, on a commitment to resolve disputes through open discussion unless compelling reasons can be adduced for restricting or concealing the policy process. Thus a general liberal political virtue is the disposition—and the developed capacity—to engage in public discourse. This includes the willingness to listen seriously to a range of views that, given the diversity of liberal societies, will include ideas the listener is bound to find strange and even obnoxious. The virtue of political discourse also includes the willingness to set forth one's own views intelligibly and candidly as the basis of a politics of persuasion rather than manipulation or coercion.

A second general political virtue is the disposition to narrow the gap (so far as is one's power) between principles and practices in liberal society. For leaders, this means admitting and confronting social imperfections through a public appeal to collective convictions. For citizens it can mean either such a public appeal or quiet acts that reduce the reach of hypocrisy in one's immediate community. For both it can lead to a tension between social transformation and law-abidingness, which can only be resolved prudentially with reference to the facts of specific cases. (This is a tension rather than contradiction between these two liberal virtues because the virtue of law-abidingness embodies not the absolute priority of the law but rather a presumption in favor of the law that can be rebutted in a narrow range of instances.)

Liberal Virtues and the Liberal Good

In the previous section I have tried to sketch a catalog of instrumental liberal virtues understood as empirical hypotheses about connections between individual character and social institutions. There is another approach that yields complementary results. The analysis of liberal theory reveals certain broad structures—in particular, a liberal theory of the good—that can enlarge our understanding of the liberal virtues.

John Rawls has tried to rest liberal theory on a "thin" account of the good as means to the attainment of whatever objectives individuals may pursue. Many critics have wondered whether *any* means can really be comprehensively neutral in the Rawlsian sense, equally serviceable for each and every goal. But the deeper difficulty lies elsewhere. Rawls's theory presupposes individuals who value their earthly existence, who give positive weight to the achievement of their purposes, and who are prepared to accept rationality as a constraint on social action and principle. More generally, every contemporary liberal theory ends up relying, covertly or overtly, on this same triadic theory of the human good: existence, fulfillment, and social rationality (Galston 1982a).[1]

This liberal theory of the noninstrumental good can serve as an independent basis for a range of instrumental liberal virtues. The worth of existence implies, first, severe strictures against cruelty and brutality as dispositions that tend to the needless destruction of existence; and second, an endorsement of the humanitarian disposition to act affirmatively to preserve the existence of others, at least when this can be done without excessive cost to ourselves. The value liberalism attaches to the fulfillment of individual purposes rests on a critique both of the otherworldliness that denigrates earthly striving in light of the hereafter and of the nihilism that withdraws all moral significance from such striving in the name of the absurdity of existence. When combined with the empirical diversity of human purposes, the liberal account of purposiveness leads directly to the vindication of tolerance, interpreted as the settled disposition of each individual to minimize interference with the strivings of others. Finally, the liberal endorsement of social rationality provides a basis for a distinctively liberal account of moderation, that is, a critique of passions and desires insofar as they weaken the disposition to confine the pursuit of individual purposes to the limits set by rational social principle.

Intrinsic Liberal Virtues

The thrust of the argument thus far has been to specify the virtues instrumental to the preservation and operation of the liberal polity. I turn now from considering liberal virtues as means to examining them as ends. The question is whether there is a conception of the virtuous or excellent individual linked intrinsically to liberal theory and seen as valuable, not instrumentally but for its own sake.

It might be thought that the answer must be negative. After all, it is characteristic of both liberal societies and liberal theories to be open to a wide variety of life plans and to their corresponding excellences. Yet the liberal tradition is by no means silent on this question. Indeed, it suggests three conceptions of intrinsic individual excellence, overlapping yet distinct.

The first is the Lockean conception of excellence as rational liberty or self-direction. As persuasively reconstructed by Rogers Smith, rational self-direction includes the capacity to form, pursue, and revise life plans in light of our personal commitments and circumstances. But it is a substantive, not merely instrumental standard: "If we value rational self-direction, we must always strive to maintain in ourselves, and to respect in others, these very capacities for deliberative self-guidance and self-control. Correspondingly, we must see the habitual exercise of these capacities as constituting morally worthy character and their enhancement as constituting morally praiseworthy action" (1985, 200).

The second noninstrumental liberal conception of individual excellence is the Kantian account of the capacity to act in accordance with the precepts of duty, that is, to make duty the effective principle of personal conduct and to resist the promptings of passion and interest insofar as they are incompatible with this principle. Judith Shklar has offered a fine sketch of Kant's morally excellent individual in action:

> At all times, he must respect humanity, the rational moral element in himself and in *all* other men. For his own sake, he must choose to avoid all self-destructive and gross behavior, and above all else, he must not lie. . . . To other men he owes no liberality or pity or *noblesse oblige* of any kind, because this might humiliate the recipients. What he does have to show them is a respect for their rights, decent manners, and an avoidance of calumny, pride, and malice. . . . This is a thoroughly democratic liberal character, built to preserve his own self-respect and that of others, neither demanding nor enduring servility. (1984, 233)

The third liberal conception of individual excellence was adapted in different ways from Romanticism by John Stuart Mill, Ralph Waldo Emerson, Henry David Thoreau, and Walt Whitman. It is the understanding of excellence as the full flowering of individuality. As Mill expounded this thesis, the excellence of individuality combines the Greek emphasis on the development, through activity, of human powers with the modern realization that the blend and balance of these powers will differ from individual to individual. Because liberal societies allow maximum scope for diversity, they are most conducive (though not perfectly so) to the development of individuality. And because liberal societies rest on individual freedom, they tend to foster the self-determination that is at the heart of true individuality. As Mill put it, "He who lets the world, or his own portion of it, choose his plan of life for him, has no need of any other faculty than the ape-like one of imitation. He who chooses his plan for himself, employs all his faculties" (1975, 56). Or as George Kateb sums up the parallel argument within the Emersonian tradition, "One must take responsibility for oneself—one's self must become a project, one must become the architect of one's soul. One's *dignity* resides in being, to some important degree, a person of one's own creating, making, choosing rather than in being merely a creature or a socially manufactured, conditioned, created thing" (Kateb 1984, 343).

Can these three distinct but recognizably liberal conceptions of human excellence be made to cohere in a single unified view? Yes, to a point. They have a common core—a vision of individuals who in some manner take responsibility for their own lives. Each links excellence to a kind of activity. And all lead to a vindication of the dignity of every individual and to the practice of mutual respect.

Beyond this common core, however, certain tensions become manifest. The exercise of Lockean rational liberty can lead to a wide range of deliberative outcomes, while Kantian duty usually prescribes a single course of conduct as generally binding. The pursuit of Emersonian individuality can tend toward a kind of poetic, even mystical self-transcendence at war with both the rationalism of Kantian morality and the prosaic, orderly self-discipline of Lockean liberty (see Rosenblum 1987, chap. 2). It is possible, of course, to resolve these tensions by giving one conception pride of place and requiring the others to maintain consistency with the preferred standard. (This is the course that Rawls—like Kant—follows when he subordinates the rational pursuit of individual life plans to the social requirements of moral right.) But it may be more advisable to accept a range of tension and indeterminacy, that is, to see the liberal polity at its best as a community that encourages all of these overlapping but distinct conceptions of individual excellence and that provides an arena within which each may be realized, in part through struggle against the others.

Intrinsic versus Instrumental Liberal Virtues

The fact that liberalism has accommodated conceptions of intrinsic as well as instrumental virtue raises the question of whether these broad categories are mutually consistent—whether (in Aristotle's terms) the liberal "good man" is the same as the liberal "good citizen." In important respects, it turns out; they are not. As Kateb observes, the Emersonian individual sees laws and institutions—including liberal-democratic institutions—as constraints on true individuality even though these very institutions are the practical presuppositions for the emergence of the ideal self. On the Emersonian account, moreover, the virtues required to maintain the liberal polity fall far short of the highest human qualities (Kateb 1984, 356–57). Similarly, even though Kant himself prescribed law-abidingness as a moral duty, the possibility of laws that command immoral action and challenge self-respect is ever present. In cases of conflict the preservation of individual integrity and dignity would seem to require that positive law give way to moral law. Finally, the Lockean doctrine of rational self-direction stands in dual tension with the requirements of liberal citizenship. Self-direction, so understood, does not readily lead to, or easily cohere with, the willingness to die for one's country. And self-direction as a maxim of individual perfection is not obviously or always consistent with a maxim of social action, binding on each individual, that commands the maximization of self-direction for all individuals throughout the community. While optimistic empirical assumptions may narrow the disjunction between the imperatives of personal perfection and social solidarity, this gap cannot be fully bridged. Indeed, it would not be farfetched to interpret some of the deepest tensions within liberal polities as a clash between means and ends, that is, between requirements of liberal citizenship and aspirations toward liberal excellence.

Engendering Liberal Virtues

It has been the burden of this article to argue that the health of liberal polities is intertwined in complex ways with the practice of what I have called liberal virtues. Now assuming that these virtues are not innate, by what means are they to be engendered? And are they adequately developed in our own liberal community? A full examination of these issues lies well beyond the scope of the present discussion. But let me conclude with three alternative hypotheses.

The optimistic hypothesis claims that daily life in the liberal polity is a powerful if tacit force for habituation to at least the minimal requirements of liberal virtue. The sorts of things regularly expected of us at home, in school, and on the job shape us in the manner required for the operation of liberal institutions. And while hardly models of moral perfection, citizens of modern liberal communities are at least adequately virtuous and not demonstrably less so than were citizens in times past.

The neutral hypothesis maintains that tacit socialization is not enough; that authoritative institutions such as families, schools, churches, the legal system, political leaders, and the media must deliberately and cooperatively foster liberal virtues; that these institutions are not now performing this task adequately; but that there is no reason in principle why they cannot do so once they come more fully to understand their responsibility.

Finally, the pessimistic hypothesis—associated with such thinkers as Daniel Bell—suggests that powerful strands of contemporary liberal culture tend to undermine liberal virtues, that in particular the various forms of liberal self-restraint have fallen victim to the imperatives of self-indulgence and self-expression (Bell 1976). From this perspective, the task of strengthening and renewing liberal virtues requires more than improving the formal institutions of moral and civic education. It requires as well a sustained effort to reverse corrosive tendencies fundamental to modern culture.

I do not know which of these hypotheses is most nearly correct. Optimists can point to the demonstrable fact that our polity has generated—or at least has not thwarted the generation of—the minimal conditions for its survival over the past two centuries. Neutralists can argue that such key social indicators as crime, drugs, family stability, and others are headed in alarming directions and that the impact of major forces of socialization—in particular, television and popular culture—is on balance negative. And pessimists can observe that with the important exception of organized religion, most sources of social authority have a diminished confidence in their ability to establish, inculcate, and enforce social norms of conduct and character.

In sorting through these competing perspectives, informal observation is no substitute for systematic, theory-guided empirical research. I cannot imagine a line of inquiry of greater significance for the conduct—and the fate—of liberal societies.

Note

1. "Any *workable* political conception of justice that is to serve as a public basis of justification that

citizens may reasonably be expected to acknowledge must count human life and the fulfillment of basic human needs and purposes as in general good, and endorse rationality as a basic principle of political and social organization. A political doctrine for a democratic society may safely assume, then, that all participants in political discussion of questions of right and justice accept these values, when understood in a suitably general way. Indeed, if the members of society did not do so, the problems of political justice, in the form with which we are familiar with them, would seem not to arise" (Rawls 1987, 4). I explore this acknowledgement's important implications for the structure of Rawls's theory and for liberal theory as a whole in Galston n.d.

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RESEARCH NOTES

UPSETTING NATIONAL PRIORITIES? THE REAGAN ADMINISTRATION'S BUDGETARY STRATEGY

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We use simulations based on a multiequation model of federal budgetary outcomes to assess the Reagan administration's impact on the federal budget during fiscal years 1982–86. Reagan's aggregate budget priorities represent a significant departure from the priorities of prior postwar administrations. The bulk of this shift in priorities had occurred by fiscal 1984. Defense spending and uncontrollable domestic spending were higher, and spending on domestic controllable programs lower under Reagan than they would otherwise have been. The distinctiveness of Reagan's budgetary priorities can be attributed to his tax cuts and—far from a strategy of "starving the budget by reducing revenues"—to a failure to allow fiscal pressures to restrain spending. The model projects that without tax cuts Reagan's predecessors would have spent no less on defense than Reagan. Cutting taxes increased the deficit by about $400 billion cumulatively during fiscal years 1982–86 and reduced expenditures by roughly $30 billion.

One of the most important goals of the Reagan administration upon its accession to power was a reorientation of U.S. budgetary, fiscal, and tax policies. The Reagan administration yielded to no one in 1981 in describing its approach to budgetary and economic policy as a break with the economic and political priorities of previous administrations.[1] The key components of this new approach were increases in defense spending, cuts in domestic spending, and cuts in taxes. In 1981, Congress approved a budget that incorporated these three components, and the Reagan "revolution" appeared to be off to a fast start.

Even as the administration won congressional approval of its budgetary and tax policies, however, critics argued that the Reagan budgetary, tax, and fiscal policies were inconsistent. How could defense spending be increased, the social safety net for the poor kept intact, major "middle-class entitlements" left untouched, and taxes cut, without creating huge deficits for the foreseeable future? The administration had two responses to this question. The first was based on the timely economic theory of "supply side economics," which seemed to show that tax cuts and investment incentives would produce rapid economic growth in the short run, increasing revenues and balancing the budget. Without debating the long-run benefits to economic prosperity of incentives for investment, it can be safely stated that the projected short-run growth in federal revenues did not materialize.[2]

A second basis for the Reagan administration strategy was the view that revenue cuts would lead to budgetary reductions. According to this "tax-spend" theory of the state, the level of available revenues drives spending; if available revenues are reduced, government spending must follow. As Meyer (1986) writes: "The government has brought pressure to bear on total outlays through the unconventional approach of scaling back available revenue. . . . Although President Reagan's strategy of pulling the revenue plug first is risky, it is better than the hopeless program-by-program fights with each constituency group against a backdrop of revenue flows that make government concessions too easy" (p. 87).[3]

We examine two questions that are central to an evaluation of the Reagan administration's budgetary strategy. First, to what degree did the Reagan administration's budgetary, tax, and fiscal policy priorities diverge from those of past administrations? Second, what evidence supports the theory behind Reagan's attempt to "starve the budget" by cutting revenues, and how successful was Reagan's application of this strategy?

Our analysis employs a model of executive and congressional budgetary decision making during fiscal years 1955–81 taken from Kamlet and Mowery 1987 to project spending levels for the fiscal 1982–86 period. Spending projections were developed under different scenarios concerning revenue policy. For each scenario, we projected spending outcomes for different budgetary categories (defense, "uncontrollable" domestic spending, and "controllable" domestic spending) for each year of the Reagan administration. We also analyzed the temporal stability of the Kamlet-Mowery model by reestimating the model leaving out, one at a time, each postwar administration since Eisenhower. The reestimated model was used to simulate spending levels for the omitted administration, allowing us to assess the robustness of the consensus concerning aggregate budgetary and fiscal priorities in the pre-Reagan period and the extent to which the Reagan administration altered these priorities.

Prior Work on the Impact of Reagan on the Budget

The budgetary priorities and programs of the Reagan administration have been scrutinized in a flood of scholarly and journalistic commentary and analysis. Many accounts describe the budgetary priorities of this administration as a significant departure from those of previous administrations. Nathan (1983) concluded that "The Reagan presidency . . . has already marked a watershed in domestic affairs. The long cycle of growth in the role and activism of the national government in domestic affairs that began with FDR's New Deal ended with Reagan's New Federalism" (p. 49). Weicher (1986) analyzed Reagan's impact on entitlement spending and concluded that "the rapid real growth of means-tested benefit programs over the previous two decades has almost been brought to a halt" (p. 7). Russett's (1982) analysis of the relationship between defense and nondefense (health and education) expenditures found that in contrast to his Republican predecessors, who promoted health and education spending as much as Democrats, Reagan cut health and education spending at the same time he expanded defense. Russett concluded that "the *Reagan* presidency *is* different" (p. 776). Butler (1985) and Mills (1984) independently attribute an increase in defense spending of approximately 1% of GNP during fiscal years 1981–85 to Reagan's influence. Mills (1984) further contended during 1981–85, nondefense spending was reduced by 1.5% of GNP below pre-Reagan policy base lines.

Other evidence, however, suggests that Reagan has not transformed national spending priorities. A study by the Congressional Budget Office (1985) concluded that the budgetary policies in effect when Reagan was elected would by fiscal 1985 have held total outlays below their actual levels by $18 billion.[4] Several scholars have argued that the buildup in defense predates Reagan (Henry and Oliver 1987; Huntington 1983) or represents a shift that would have occurred, at least to some degree, under other administrations (Glazer 1984; Mills 1984; Ostrom and Marra 1986).[5] Other analyses of domestic spending claim that cutbacks in spending on programs for the low-income population predate Reagan. Nathan (1983) argues that "retrenchment had been gaining strength in domestic policy since the mid-seventies" (p. 48), as does Sawhill (1986).

Still other evidence supports skepticism about the magnitude of the shift in spending priorities under Reagan, especially in domestic programs. Total nondefense spending, for instance, has not declined under the Reagan administration, measured either in real terms or as a percentage of the GNP. Indeed, Butler (1985) found that transfer payments to individuals rose as a percentage of GNP during the first part of the Reagan administration. By 1985, the architect of the Reagan budgetary policy, David Stockman, characterized this policy as a failure (Stockman 1986), due to the inability of the administration to cut domestic spending.

Other analysts have questioned the novelty of Reagan's tax policy. Galbraith (1986) argues that the 1981 tax cut represents a change in the timing, rather than the substance, of the revenue strategies of prior administrations: "Up until the third quarter of 1983 the deficit was very near what would have been expected from the behavior of past administrations confronted with the same economic conditions that Reagan faced" (p. 21). According to Galbraith, previous administrations would have responded to the 1981–82 recession with a tax cut comparable in size to that in 1981, although the tax reductions that took effect in mid-1983 were greater than predicted from data on previous administrations. In this view, Reagan's only significant departure from the postwar pattern of revenue policy was in the second- and third-year tax reductions contained in the 1981 bill.

Clearly, there remains substantial controversy about the actual impact of the Reagan administration on budgetary and fiscal priorities. In part, this reflects the fact that most empirical analyses have examined the various components of federal spending and revenue policy in isolation from one another and have not compared the Reagan budgetary and tax policies against a "counterfactual" model of budgetary and fiscal outcomes. A thorough assessment of Reagan's impact on the budget must analyze the joint effects of Reagan spending and tax policies and must specify a clear counterfactual scenario in doing so.

The Basic Model

The basic model in Kamlet and Mowery 1987 can be used to assess Reagan's effect on total spending and taxes and impact on spending priorities by comparing his policies with those that would have been observed had the budgetary priorities of past administrations been in effect during the past six years. The Kamlet-Mowery model is a multiple-equation, reduced form model of budgetary outcomes that examines three categories of expenditures: defense outlays, "controllable" domestic outlays, and "uncontrollable" domestic outlays (excluding interest payments).[6] A fourth category of expenditures—interest payments—is embedded in the model but is treated as exogenous. Each equation in the Kamlet-Mowery model allows for three types of influences on spending: influences that are specific to the spending category being examined (in the case of defense spending, for instance, these influences included the Soviet threat and the level of armed conflict); total revenues; and spending pressures in other components of the budget. The model treats revenues as unresponsive to spending pressures, at least in the short run, a feature that merits additional discussion.

The Kamlet and Mowery model allows revenue and fiscal pressures to influence spending outcomes but assumes that spending pressures do not affect revenues. The parameter estimates of the model are not strongly affected by this assumption, however.[7] Similar assumptions have been employed in other recent papers (Auten, Bozeman, and Cline 1984; Fischer and Crecine 1981; Fischer and Kamlet 1984).[8]

Empirical attempts to test the causal relationship between spending and taxation have yielded mixed results (see, e.g., Anderson, Wallace, and Warner 1986; Blackley 1986; Furstenberg, Green, and Jeong 1985, 1986; and Congressional Budget Office 1987, Manage and Marlow 1986, Vedder, Gallaway, and Frenze 1987). This is not surprising, given the small number of data points in studies of year-to-year spending and revenue trends and the methodological difficulties in determining the existence and direction of a causal link between these two variables. Kamlet, Mowery, and Su (1988) assess the robustness of these empirical studies, applying univariate and multivariate Granger tests to alternative specifications for the 1953–80 period. They conclude that with a few exceptions (e.g., the 1969 Vietnam surtax), U.S. tax policies were independent of spending pressures during the postwar period prior to Reagan but that spending was heavily affected by revenue availability. If the assumption that revenues affect spending and not the reverse is incorrect, our model will overstate the influence of revenues on spending. Since the Reagan administration's budgetary strategy assumes the existence of a close link between revenue and spending decisions, the model will err in the direction of supporting the validity of the Reagan budgetary strategy.

Kamlet and Mowery (1987) provide a detailed discussion of the model and estimates of its parameters for fiscal years 1955–81.[9] Several of the estimation results are of particular interest for this analysis. First, the relatively high goodness of fit of the estimating equations for the entire period, as well as the lack of statistical significance of administration-specific dummy variables, suggests that aggregate budgetary priorities were relatively stable from Eisenhower through Carter, although both entitlement spending and the size of the deficit grew steadily during this period. Traditional political party variables had little impact on these budgetary priorities in either the White House or Congress. These results support the view that there was a general consensus on aggregate budgetary and fiscal policy priorities during 1955–81 that spanned Republican and Democratic presidencies. A final conclusion of relevance for this work is that the revenue environment and spending pressures in other spending programs constrain growth in the defense budget but exert much less influence on domestic controllable and uncontrollable spending.

The Simulations

A Simulation Assuming the Reagan Tax Changes

The parameter estimates from the model for fiscal years 1955–81 were used to simulate the budget for fiscal years 1982–86. The results of the simulation represent budgetary priorities that would have been obtained for fiscal years 1982–86 if these priorities and interdependencies had remained unchanged from those of the 1955–81 period; that is, the simulation assumes a "composite" president and Congress based on those of the 1955–81 period and projects budgetary outcomes under such a president and Congress during 1982–86. In each case, the dependent variable is outlays after congressional action on the budget.

The first simulation is reported in the second column of Table 1. It is a "successive" simulation. Budgetary outcomes first are projected for fiscal 1982. These simulated values for fiscal 1982 are used to simulate fiscal 1983, and the procedure is extended through fiscal 1986. Because the basic model takes interest payments as exogenous, simulated and actual interest payments are identical. This first simulation also employs actual 1982–86 federal revenues. It therefore reflects the tax changes that took place during the Reagan administration. The projected values are expressed both in dollar terms and as a percentage of GNP. The first simulation uses as macroeconomic variables (inflation and unemployment) the actual economic conditions during 1982–86.[10]

The simulation is subject to three sources of uncertainty. First, each equation in the Kamlet-Mowery model has an additive error term. Second, the parameters from the 1955–81 period are themselves only estimates and contain error. Third, in a successive simulation of an equation system like that employed here, some of the predetermined variables are simulated (e.g., prior year outlay levels), adding an additional stochastic component. It is generally impossible to derive analytically tractable expressions for

Table 1. Simulation Results

Fiscal Year	Simulation 1	Simulation 2	Actual Outlays[a]
	Defense Outlays		
81	—	—	158 (.052)
82	159 (.050) [.042, .058]*	174 (.055) [.047, .063]	183 (.058)
83	171 (.050) [.038, .061]*	204 (.060) [.047, .073]	209 (.061)
84	161 (.043) [.031, .053]*	209 (.056) [.042, .068]	231 (.061)
85	174 (.044) [.033, .053]*	233 (.058) [.042, .069]	246 (.062)
86	203 (.048) [.035, .060]*	273 (.065) [.051, .078]	258 (.061)
	Controllable Domestic Outlays		
81	—	—	146 (.048)
82	156 (.049) [.040, .059]	157 (.050) [.040, .060]	133 (.048)
83	184 (.054) [.042, .067]*	186 (.055) [.041, .069]*	132 (.039)
84	197 (.052) [.040, .064]*	199 (.053) [.040, .065]*	146 (.039)
85	209 (.052) [.042, .063]*	210 (.053) [.040, .066]	168 (.042)
86	229 (.054) [.042, .068]*	228 (.054) [.040, .070]*	160 (.038)
	Uncontrollable Outlays		
81	—	—	307 (.101)
82	329 (.104) [.096, .112]	330 (.104) [.095, .112]	342 (.108)
83	366 (.107) [.096, .119]	367 (.108) [.094, .119]	395 (.116)
84	389 (.103) [.093, .113]	390 (.103) [.089, .116]	386 (.102)
85	406 (.102) [.090, .111]*	405 (.101) [.088, .111]*	447 (.112)
86	425 (.101) [.089, .112]	422 (.100) [.085, .112]	456 (.108)

Table 1. (continued)

Fiscal Year	Simulation 1	Simulation 2	Actual Outlays[a]
		Interest	
81	—	—	80 (.026)
82	99 (.031)	99 (.031)	99 (.031)
83	105 (.031)	105 (.031)	105 (.031)
84	128 (.034)	128 (.034)	128 (.034)
85	156 (.039)	156 (.039)	156 (.039)
86	172 (.041)	172 (.041)	172 (.041)
		Total Outlays[b]	
81	—	—	691 (.226)
82	742 (.235)	760 (.240)	757 (.239)
83	826 (.243)	862 (.253)	841 (.247)
84	875 (.232)	926 (.245)	891 (.236)
85	945 (.237)	1,003 (.251)	1,017 (.255)
86	1,029 (.244)	1,095 (.260)	1,046 (.248)
		Total Revenues[c]	
81	—	—	636 (.208)
82	659 (.208)	700 (.221)	659 (.208)
83	633 (.186)	708 (.208)	633 (.186)
84	707 (.187)	806 (.214)	707 (.187)
85	795 (.199)	906 (.227)	795 (.199)
86	837 (.198)	967 (.229)	837 (.198)
		Deficit	
81	—	—	55 (.018)
82	83 (.026)	60 (.019)	98 (.031)
83	193 (.057)	154 (.045)	208 (.061)
84	168 (.045)	120 (.032)	184 (.049)
85	150 (.038)	97 (.024)	222 (.065)
86	192 (.046)	128 (.030)	209 (.050)

Note: The first figure in each row represents outlays in billions of dollars. Figures in parentheses are outlays as a share of GNP. Figures in brackets reflect 95% confidence interval for share of GNP.

[a] Outlays after congressional action on the budget.

[b] Total outlays as reported here equal "total outlays" as listed in the *Budget of the United States Government* apart from undistributed offsetting receipts.

[c] Total revenues as reported here equal "total receipts" as listed in the *Budget of the United States Government* plus undistributed offsetting receipts.

simulation confidence intervals for equation systems estimated sequentially and containing simulated predetermined variables.

These difficulties were overcome through the use of Monte Carlo methods to generate confidence intervals for the simulation results. We specified a joint distribution for the parameters of each equation in the basic model using the parameters' variance-covariance matrix from the 1955–81 estimation. Similarly, we specified a distribution for the additive error term of each equation using the estimated variance of each error term from the 1955–81 estimation. Using these distributions, a set of parameter values and additive error terms were drawn at random for each equation and used in a deterministic successive simulation. For each year of the simulation, a new additive error term was drawn for each equation. A total of one thousand such simulations were run for each budget category, with a 95% confidence interval constructed for each year and each budgetary category.[11]

The results in Table 1 show that defense spending under Reagan substantially exceeds the levels projected by the simulation. In each year this gap is statistically significant; that is, given prevailing revenues, defense outlays in fiscal 1986 substantially exceeded the level that would have been achieved by this fiscal year had the pre-Reagan budgetary and fiscal priorities applied. By fiscal 1986, actual defense spending was $55-billion greater than projected by the simulation, an increase in defense spending of nearly 25%.[12] Interestingly, this excess of actual over predicted defense spending was attained in its entirety by fiscal 1984.

Controllable domestic spending under Reagan is lower than would otherwise have been the case. The difference is statistically significant for four of the five years.[13] The difference for fiscal 1986 between actual and simulated outlays is $69 billion, or nearly 33% of predicted controllable domestic outlays. As with defense spending, most of the gap between predicted and actual domestic controllable spending emerged by fiscal 1984.

Surprisingly, uncontrollable spending (excluding interest) under Reagan exceeds the level projected by the simulation for four of the five years analyzed. The difference is statistically significant for only one year. The sum of uncontrollable spending for fiscal years 1982–86 as a proportion of GNP, .5464, however, exceeds the 95% confidence level for this sum (.4918, .5380). In 1986, actual uncontrollable outlays exceed predicted levels by $31 billion, or over 5% of actual uncontrollable spending. The results of this simulation suggest that the Reagan administration's spending policies served to reinforce one of the central federal budgetary trends of the past three decades: growth in uncontrollable spending. More disaggregated analyses of the changing structure of domestic uncontrollable spending during the Reagan years (see Boskin 1987; Meyer 1986; Mills 1984) suggest that the administration's unwillingness or inability to impose greater cuts in so-called "middle-class" entitlements (Social Security, Medicare, veterans' pensions), coupled with congressional resistance to even more stringent reductions in entitlements for the poor and working poor (AFDC, food stamps), supported continued growth in domestic uncontrollable spending above the levels permitted by the spending and fiscal priorities of Reagan's predecessors.[14]

Total spending under Reagan is $17-billion higher than the level projected by the simulation for fiscal 1986, consistent with the findings of the Congressional Budget Office study (1985). A similar conclusion applies to the deficit. Under the first simulation the fiscal 1986 deficit is $192 billion, compared to the actual fiscal 1986 deficit of $209 billion. The simulation suggests that tax cuts of the size passed during the Reagan administration would under other administrations have produced *greater* reductions in total spending than was true under Reagan. Had the Reagan administration followed the spending priorities of its predecessors, military and domestic uncontrollable spending would have been cut more severely in response to the reductions in taxes, while domestic controllable spending would have been reduced by a smaller amount.

Moreover, since lower deficits would reduce interest payments, the simulated total overstates total outlays. The model used for the simulations takes interest payments as exogenous and equal to their actual levels during 1982–86. Budget deficits in the simulations, however, are lower than the actual deficits. Assuming an average interest rate on government debt of 6% during 1981–86 and compounding, interest payments in fiscal 1986 would be lower than their actual level by about $8 billion. The simulated level of total spending in fiscal 1986 thus would be smaller than actual spending by more than $25 billion.

A Counterfactual Simulation Assuming No Reagan Tax Changes

The results of an alternative simulation are also reported in the third column of Table 1. This second simulation employs Congressional Budget Office projections of the level of revenues that would have prevailed during 1982–86, given actual macroeconomic experience, in the absence of the fiscal 1981 tax cuts. This simulation depicts spending outcomes if the spending priorities of prior presidents had been applied without the Reagan tax cuts.

The second simulation yields conclusions concerning the impact of Reagan on uncontrollable and controllable domestic spending that are similar to those of the first simulation. Uncontrollable spending is higher, and domestic controllable spending lower under Reagan than would have been true under his predecessors, given similar revenue environment. The conclusions of the two simulations differ, however, for defense spending. The second simulation predicts that defense spending would have *exceeded* its actual 1986 level had Reagan not cut taxes. The simulated levels of defense spending for fiscal 1986 under the second simulation is $273 billion, $15-billion greater than the actual level of $258 billion.[15]

The explanation for the contrasting results of these simulations concerning defense spending is straightforward. Prior to Reagan, defense was more responsive to the revenue environment and to spending pressures from other parts of the budget than were other budgetary components. Defense spending was relatively sensitive to "top-down" spending pressures and to fiscal policy concerns. The less stringent revenue environment in this second simulation could support faster growth in defense spending during fiscal years 1981–86 than was observed under Reagan.[16]

Total spending under the second simulation exceeds that of fiscal 1986 by $49 billion (about 5% of total budgetary outlays, over 1% of GNP). Thus, by cutting taxes and "starving the budget," Reagan shrank total spending by about $50 billion by fiscal 1986—or by $94 billion for the entire 1982–86 period—below the levels that would have prevailed under his predecessors. These spending reductions, however, were far too small to offset the impact on the deficit of the 1981 tax cut. The fiscal 1986 deficit predicted by the second simulation is $128 billion, well below the actual fiscal 1986 deficit of $209 billion. The cumulative amount by which the actual deficit exceeds that predicted by the simulation for fiscal years 1982–86 is $362 billion. Moreover, when the comparison is adjusted to incorporate higher interest outlays (more than $22 billion by fiscal 1986), Reagan's actual spending cuts shrink to $28 billion for fiscal 1986 and the gap between the simulated and actual deficit expands to $103 billion for fiscal 1986 and to a cumulative total of $398 billion for fiscal years 1982–86.

The results from the second simulation are largely consistent with the conclusions from a comparison of the Reagan budget with the long-run projections (fiscal years 1983–86) in President Carter's fiscal 1982 budget, which of course did not assume tax cuts of the size of those of 1981 (Office of Management and Budget 1981a). Defense spending was projected by Carter to

Table 2. Cumulative Residuals

Administration	Defense	Controllable	Uncontrollable
		With Actual Tax Changes	
Eisenhower	.0035	-.0140	-.0075
Kennedy	.0041	-.0027	.0018
Johnson	-.0085	.0038	.0102
Nixon	-.0025	-.0081	.0261
Ford	-.0058	.0050	-.0054
Carter	-.0022	.0032	-.0182
Reagan	.0130	-.0163	.0072
		With No Tax Changes	
Eisenhower	.0041	-.0098	-.0089
Kennedy	.0060	-.0035	.0016
Johnson	-.0113	.0053	.0130
Nixon	-.0085	-.0050	.0216
Ford	.0002	.0003	-.0039
Carter	.0031	.0030	-.0168
Reagan	-.0035	-.0162	.0080

Note: Residual = actual − projected (measured as a share of GNP).

be 5.9% of GNP in fiscal 1986, nearly as high as the actual level of 6.1% in the fiscal 1986 budget after congressional action. Carter projected uncontrollable spending to equal 9.1% of GNP by 1986, compared to 10.8% in the fiscal 1986 budget after congressional action. Controllable spending was projected by Carter to be 4.6% of GNP by 1986, compared to 3.8% in the fiscal 1986 budget after congressional action. As in the second simulation analysis, Carter's projections for fiscal 1986 produce a budget with defense spending that is comparable to the Reagan defense budget, less uncontrollable spending than was contained in the Reagan budget, and more controllable spending.

Is Reagan the Only Outlier?

The simulation analysis suggests that aggregate budgetary priorities during the Reagan administration differ substantially from the "composite president and Congress" of the Eisenhower-to-Carter period, particularly when the 1981 tax cuts are included. This analysis, however, does not assess the extent to which the Reagan administration's departure from previous norms is unique. Have other postwar administrations departed from historical patterns to a comparable extent, albeit in other directions?

We analyzed this question by performing simulations for presidencies other than that of Reagan. For each presidency from Eisenhower to Carter, the basic model was estimated using data from the other administrations (including the Reagan administration). The resulting parameter estimates were then used to generate successive simulations for each year and each category for each administration. These results are summarized in Table 2, which reports the cumulative residual, i.e., the difference between actual and predicted values at the end of each administration, for each budgetary category. These residuals, measured as a share of GNP, provide a measure of the extent to which each administration departed from the priorities of other administrations.

The first half of Table 2 presents residuals calculated for the actual 1982-86 tax regime. The residual during the Reagan years for defense is by far the largest for that category.[17] The average absolute value of the residual for defense from Eisenhower through Carter is .44% of GNP, compared to a positive 1.30% for Reagan. The Reagan residual for controllable domestic spending also has the largest absolute value. The average absolute value of the residual for Eisenhower through Carter for controllable domestic spending is .61% of GNP, considerably below its (negative) value of 1.63% for Reagan. The absolute value of the Reagan residual for uncontrollable spending, however, is less than those of four other administrations, two of which, Johnson and Nixon, have a positive value. The average absolute value of the residual for Eisenhower through Carter is 1.15% of GNP, compared to a positive value of .72% for Reagan. The analysis in the top of the table indicates that Reagan altered budgetary priorities to a substantially greater degree in defense and controllable domestic spending than past administrations but also suggests that he was much less unusual with regard to uncontrollable spending.

The bottom panel of Table 2 presents residuals calculated from simulations that assume no tax changes under Reagan. These results contrast sharply with those in the top panel. When the revenue environment of the Reagan years is constrained to resemble that of previous presidents, the size of the Reagan residual for defense spending is not exceptional, declining from 1.30% of GNP in the top to −.35% of GNP in the bottom. Consistent with our discussion above, the distinctive feature of the Reagan military buildup is the fact that it occurred in the context of large tax cuts and budgetary deficits. If taxes had not been cut, this analysis suggests that past administrations would have instituted a defense buildup of comparable scale. Analysis of the residuals for uncontrollable spending and controllable domestic spending yields results that are similar to those in the top panel of the table. Reagan's cuts in controllable domestic spending stand out, although the increases in uncontrollable spending conform broadly with the budgetary behavior of prior administrations.

This analysis of simulations for each administration yields two important conclusions. Aggregate budgetary priorities in the United States appear to have been relatively stable during the postwar period before Reagan. The average absolute value of the residuals in the bottom of Table 2 for Eisenhower through Carter are .55% of GNP, 1.10% of GNP, and .45% of GNP for defense, uncontrollable, and controllable domestic spending, respectively. These figures, in turn, represent 11%, 11%, and 9% of total spending in these categories (based on fiscal 1981 spending levels). Averaged across the tenure of each administration from Eisenhower to Carter, budgetary outcomes deviated from the levels predicted by the simulation by roughly 2% per year in any single budgetary category.[18] The ability to shift spending 2% per year within aggregate budgetary categories suggests some degree of discretion but hardly is evidence of radical shifts in aggregate budgetary priorities in the postwar period prior to Reagan.

Second, consistent with Russett 1982, this analysis suggests that Reagan *is* different. The deviations of budgetary outcomes from predicted levels under the Reagan administration are substantially larger than those of prior presidencies, with a buildup in defense and uncontrollable spending and cuts in controllable domestic spending. The increases in defense spending, however, are unusual mainly because they occurred simultaneously with large tax cuts, rather than representing increases that were unprecedented in absolute magnitude or greater than those expected in a different revenue environment.

Presidential Impacts and Congressional Impacts

Faced with unprecedented deficits during his administration, Reagan has frequently argued that Congress has refused to go along with the budgetary priorities reflected in the president's budget. We have examined actual and predicted outlay levels that incorporate the effects of congressional action on the budget. The dependent variable thus incorporates the budgetary impacts of both the executive branch and Congress and does not separate the impact of the executive branch on the budget from that of Congress.

Although a large body of work in political economy, particularly the analysis of "political business cycles," has attributed budgetary outcomes solely to the White House, both the president and Congress affect budgetary outcomes, and the influence of each branch on outcomes is difficult to separate. In past work (Fischer and Kamlet 1984; Kamlet and Mowery 1987), executive branch action was assumed to be captured in the president's budget proposal. Unfortunately, this assumption yields imperfect measures of presidential intentions or ultimate presidential impact. The executive branch obviously influences the budget after its submission to Congress. Particularly under Reagan, the president's budget has sometimes been the first play of a multistage game, a document that establishes an initial bargaining position with Congress or yields favorable publicity with the electorate. As such, the policy positions in the president's budget may be more extreme than "true" presidential policy preferences. Conversely, anticipated congressional actions may temper presidential requests, leading to changes in the president's budget that accommodate congressional goals, rather than expressing those of the White House.

Despite these difficulties of measurement, an analysis of the president's budget requests provides one means for analyzing the budgetary dimensions of presidential interactions with Congress. Kamlet and Mowery (1987) estimate a version of their model using oulays levels in the president's budget as the dependent variable. This model of the president's budget proposals can be used to generate simulations like the ones presented.

The Kamlet and Mowery (1987) model was used to simulate the president's budget for each year and budget category, assuming the tax changes that actually occurred under Reagan. This was not done sequentially, as were the other simulations, but separately for each year, reinitializing each year based on actual congressional action from the previous fiscal year.[19] We ran a similar simulation for outlays after congressional action.

For 1982-86, defense spending in the president's budget exceeded predicted levels by an average amount of $34 billion. The defense budget after congressional action was on average $39-billion greater than predicted by the simulation of the budget after congressional action. Thus, during fiscal years 1982-86, defense spending after congressional action exceeded historical norms by as much as or more than defense spending proposed by the president. The president's budget for uncontrollable spending was on average $8-billion greater than its simulated value, while uncontrollable spending after congressional action was $13-billion greater than the simulated value. In other words, uncontrollable spending after congressional action was slightly further from historical norms than was the president's budget. Finally, controllable spending was $19-billion less on average in the president's budget than in the simulation of president's budgets, while controllable spending was $29-billion less on average after congressional action than in the congressional action simulation. Measured relative to historical spending norms, congressional decisions on controllable spending represent deviations that are even greater in magnitude than those requested by the White House. Rather than offsetting presidential budget requests, Congress appears to have accommodated White House budget requests for controllable spending.

For defense and controllable domestic spending, outlays after congressional action deviated from simulated values by as much or more than the deviation in the president's budget. This is not to say that Congress always passed the budgetary allowances recommended by the president during this period. For instance, Congress cut Reagan's defense proposal by an average of $11.8 billion during this period, increased controllable spending by $14.4 billion on average, and increased uncontrollable spending by $15.2 billion on average. Such congressional changes in the president's budget, however, are typical of executive-legislative budgetary interactions. The Reagan administration apparently convinced Congress to move its budgetary priorities as far from its pre-1981 priorities as Reagan's presidential budgets differed from pre-1981 president's budget priorities. The claims by Reagan that Congress is responsible for the deficit and for the shifts in the composition of the budget are not easy to square with this analysis. Congress has if anything been less assertive in revising presidential spending proposals during fiscal years 1982–86 than was true before Reagan.

Conclusion

This analysis suggests that although the Reagan administration has had a major impact on the composition of the budget, the strategy of "starving the budget" through tax cuts has been a mixed success at best. As measured by our basic model, aggregate budgetary priorities in the United States have been relatively stable from Eisenhower through Carter. The Reagan administration, however, appears to have departed dramatically from the broad budgetary priorities of other administrations. In particular, our analysis indicates that defense spending was higher and controllable domestic spending substantially lower under Reagan than would have been the case under pre-Reagan budgetary priorities. Most of this gap emerged in the first three years of the Reagan administration. Moreover, congressional budgetary decisions during fiscal years 1982–86 largely ratified the Reagan departure from historical spending norms.

At the same time, the simulation analysis suggests that Reagan's spending priorities directly conflicted with his strategy of reducing total spending through tax cuts. Defense spending is the area of the budget that has traditionally been most responsive to revenue pressures. The distinctive feature of the Reagan military buildup is the fact that it occurred in the context of large tax cuts and budgetary deficits. If taxes had not been cut, our analysis suggests that past administrations would have instituted a defense buildup of comparable scale. The Reagan administration's reluctance to let the tax-spend logic work in the one area of the budget in which it has been strongest—combined with an inability to reduce domestic uncontrollable spending—meant that total expenditures were not reduced significantly by deficit pressures. The budgetary "diet," imposed only on domestic controllable spending, could not offset the large revenue losses of 1981 and subsequent years. By fiscal 1986, total expenditures may have decreased by as little as $28 billion as a result of the tax cuts, despite an additional cumulative $398 billion in deficits between 1982 and 1986.

One effect of the Reagan budget strategy was the transformation of domestic controllable budgeting from an "independent process" (Kamlet and Mowery 1987), unconstrained by fiscal pressures and spending pressures from other programs, to one that apparently is far more constrained by "top-down," revenue-based spending constraints. This conclusion is consistent with the analysis of the Reagan budgets by Schick (1983) and Bozeman and Straussman (1982), which argue that "top-down" budgeting (Kamlet and Mowery 1980) has become far more important under Reagan. Such controllable domestic spending, however, represents only a small proportion of the overall budget. More significant is the opposite transformation under Reagan of defense spending into a budgetary policy stream relatively unresponsive to fiscal pressures. Reagan dissolved the link between revenue and spending decisions, and in so doing, appears to have flouted the dietary theory of budgeting far more systematically than his postwar predecessors.

Notes

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1. The *Fiscal Year 1982 Budget Revisions* (Office of Management and Budget 1981b) released on 10 March 1981, described the Reagan budgetary program as "based on the belief that Government policy must again become reliable and consistent. The approach embodied in the Administration's plan is a major departure from past policies, which consisted of rapid growth of Federal spending, excessive increases in the money supply, frequent changes of direction, historically high and continually rising tax burdens, and unwarranted regulation" (p. 1).

2. Although adherents of supply side economics often attribute this shortfall to actions of the Federal Reserve System, we do not consider the supply side theory in more detail here.

3. Similar arguments are made by Moynihan (1988), von Hayek (cited therein), and Lawrence H. Summers, Interview by Karen Anderson, *Stanford Magazine* (Spring 1987), pp. 21-27.

4. Congressional Budget Office 1985 (p. 155). The study projected total spending of $920 billion in fiscal 1985 under policies in effect when Reagan was elected, compared to $938 billion in actual fiscal 1985 spending.

5. In April of 1986, the *New York Times* ran an article entitled "Reagan Acknowledges That Carter Had Proposed a 5-Year Military Buildup." The story reports a conciliatory speech by Reagan addressing former President Carter's charge that "Mr. Reagan 'habitually' misstated his record on military modernization." In the speech Reagan acknowledged that "my predecessor had called attention to the decline of military spending in the mid-1970's and had proposed a five-year expansion of the defense budget."

6. Uncontrollable domestic outlays consist of Medicare, Medicaid, Social Security, Civil Service Retirement benefits, Railroad Retirement benefits, veterans' pensions, unemployment compensation, AFDC, food stamps, school lunch programs, farm income stabilization programs, and General Revenue Sharing.

7. Kamlet, Mowery, and Su (1988) report the results of two-stage least squares estimation, which allows for spending outcomes to influence revenues and vice versa. The results of the two-stage least squares estimation are comparable to those obtained using ordinary least squares.

8. Schick (1980), Mowery, Kamlet, and Crecine (1980), and Schier (1987) portray the process of budgetary and tax policies as "decoupled," with tax policy not responding to spending pressures.

9. The model results reported here differ slightly from those in Kamlet and Mowery 1987. We omitted the "perceived Soviet threat variable" since we do not have measures of this variable for the Reagan period. We also included two additional programs—General Revenue Sharing and farm price support programs—in uncontrollable domestic programs.

10. In using actual economic conditions and actual revenues in the simulations, we ignore any secondary feedback effects, in which Reagan fiscal, tax, and budgetary policies affect macroeconomic performance, which in turn affects subsequent Reagan fiscal, tax, and budgetary policies. To the degree these feedback effects are important, the conclusions of the simulation analysis must be qualified.

11. See Pindyck and Rubinfeld 1981 (pp. 405-10) for a more detailed discussion of this methodology.

12. Measuring total defense spending from fiscal

1982 to 1986 as a percentage of GNP, the 95% confidence level is (.207, .258), compared with actual spending of .3032.

13. Measuring controllable domestic spending from fiscal 1982 to 1986 as a percentage of GNP, the 95% confidence level is (.236, .291), compared with actual spending of .1995.

14. Meyer (1986) noted that "ironically, although the administration's pronouncements stressed fiscal control and the need to protect only the truly needy, its policy decisions during the first term sacrificed fiscal control to the maintenance of a much broader system of both public expenditures and tax subsidies flowing to all income groups" (p. 73). Mills provides a similar assessment (1984, 121–23). Boskin concluded that "the reduction in means-tested benefits payments has been more than offset—in both absolute dollars and growth rates as a share of the budget and GNP—by growing benefit payments in programs that are not means-tested" (1987, 134).

15. Measuring total defense spending from fiscal 1982 to 1986 as a proportion of GNP, the actual level of .3032 is well within the associated 95% confidence level for the second simulation of (.2662, .3191), suggesting that this difference is not statistically significant.

16. To further examine the relationship of revenues and defense spending under Reagan, we ran the basic Kamlet-Mowery model for the period 1955–86, inserting a dummy variable for Reagan and interacting the Reagan dummy with the revenue term. Using defense outlay levels in the president's budget as the dependent variables, we found a statistically significant coefficient for the Reagan dummy variable and a negative, statistically significant coefficient for the interaction term of the same magnitude as the (positive) coefficient for revenues. Using defense outlays after congressional action as the dependent variable led to very similar coefficient estimates but ones with statistical significance below conventional levels. This result is consistent with the conclusions of the simulation analysis, which also indicate that Reagan expanded defense and dissolved the link between revenues and defense spending.

17. The relatively large negative sign for Johnson must be interpreted with some caution. Defense budgets in the model are heavily influenced by combat deaths. When the model is estimated excluding the Johnson administration, yielding the residual values for that presidency, the influence of combat deaths on the military budget is affected mainly by the Korean War and the Vietnam War under Nixon. The negative sign of the residual for the Johnson years (mainly 1966–68), during which combat deaths were relatively high, indicates that there was greater budgetary sensitivity to changes in combat intensity during the Korean War and the latter part of the Vietnam War than during the military buildup in Vietnam.

18. This estimated flexibility of 2% per year is slightly more conservative than LeLoup's (1978) assessment that the "actions of the president and Congress can be evaluated as contracting, maintaining, or expanding the current services budget, but only within a range of about plus or minus 5% of the total in normal years" (LeLoup 1978, 462–63).

19. Cumulative simulations are less useful for the analysis of executive-congressional budgetary differences than these reinitialized simulations, since in our model the president's budget request is influenced by the budgetary decisions made during the previous year by Congress.

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PRESIDENTIAL PRENOMINATION PREFERENCES AND CANDIDATE EVALUATIONS

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Recent research has altered our understanding of how voters select a candidate in U.S. presidential elections. Scholars have demonstrated empirically that issues, candidate personalities, candidate evaluations, and party identification interact in a dynamic simultaneous fashion to determine vote choice. Other researchers have shown that prenomination candidate preferences play an integral role in structuring the general election vote. We join together these two important trends to introduce and test a revised model of vote choice, using 1980 NES panel data. The results reconfirm that candidate selection is part of a dynamic simultaneous process and reveal for the first time that prenomination preferences are woven tightly into this causal web.

The empirical study of vote choice in presidential elections entered a new era in the 1970s. Although the principal research question—What determines vote choice?—remained the same, significant methodological advances spawned a flurry of research that altered our understanding of the decision-making process culminating in the vote. Beginning with Jackson (1975), scholars used newly developed two-stage least squares regression to confirm what earlier researchers (Campbell et al. 1960; Goldberg 1966) could only hypothesize: that the forces structuring presidential vote choice were related simultaneously to one another and to the vote (Fiorina 1981; Markus 1982; Markus and Converse 1979; Page and Jones 1979). Along with employing two-stage least squares, Markus and Converse (1979), Fiorina (1981), and Markus (1982) also adjusted their models to reflect the dynamic nature of the vote choice process. These scholars recognized that the attitudes and beliefs that influence candidate selection are the product of previously held attitudes and beliefs and thus that a properly specified model of vote choice must incorporate this dynamic structure. So by the early 1980s the act of voting for a presidential candidate was seen as the end result of a dynamic simultaneous interplay of forces.

At about the same time that scholars began developing dynamic simultaneous models of vote choice, the presidential selection process underwent substantial change. Starting with the 1972 campaign, the locus of power during the nomination process shifted from the party elite to the rank-and-file electorate. This change transformed presidential campaigns from primarily two-candidate affairs commencing on Labor Day weekend to multicandidate events spanning a year or more. As such, models of vote choice that focused on individuals' attitudes toward general election candidates to the exclusion of at-

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titudes toward the major prenomination contenders may have missed an important determinant of the vote; that is, it seems quite plausible that individuals' orientations toward presidential hopefuls at the prenomination stage may influence general election choices.

Unfortunately, the dynamic simultaneous models of vote choice developed to date have failed to incorporate a term tapping individuals' attitudes toward prenomination contenders. The presidential nomination reforms did, however, spark interest in what has commonly been called the "carryover" hypothesis (Stone 1986). The hypothesis contends that prenomination battles often leave supporters of losing candidates bitter toward their party's eventual nominee, perhaps leading some actually to prefer the opposition party's candidate in the general election. Scholars searching for such a link between prenomination candidate preferences and general election preferences in presidential campaigns have found supporting evidence. Stone (1986), Southwell (1986), and Lengle (1980) used individual-level data to demonstrate that on average, people who supported candidates who failed to win the nomination were less likely to support their party's nominee in the fall election than individuals who backed the eventual nominee during the primary season. Kenney and Rice (1987) and Lengle (1980) corroborated these findings with aggregate-level data. The weight of the evidence, then, suggests that prenomination preferences influence general election vote choice. Regrettably, though, a review of the carryover research reveals that no attempt was made to incorporate prenomination preferences into a fully specified dynamic simultaneous model of the November vote.

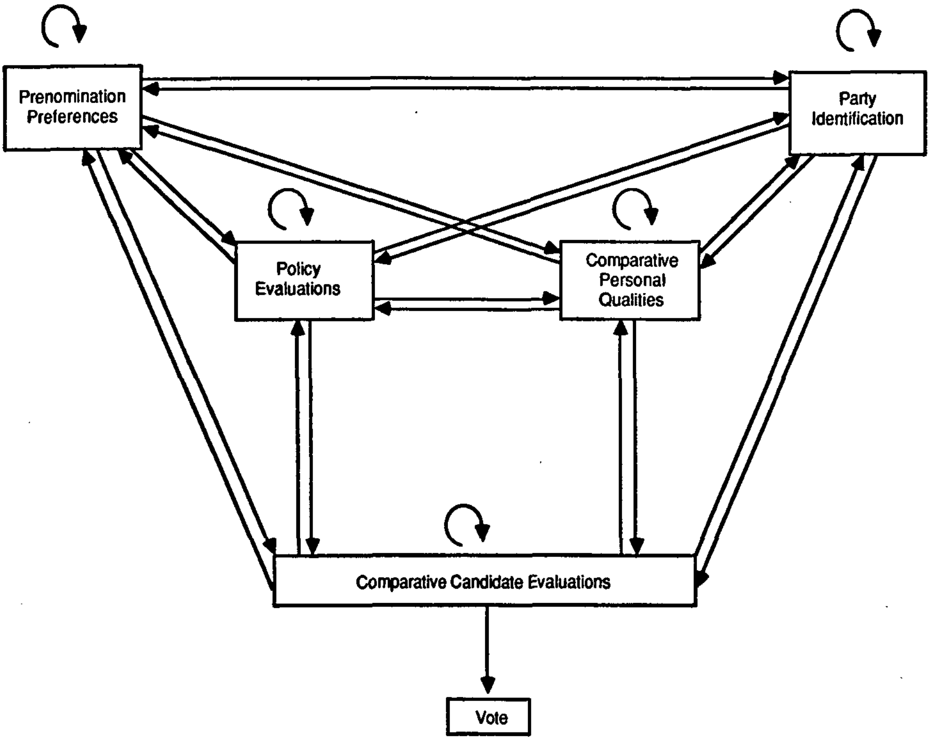
The pioneers of the dynamic simultaneous models of voting failed to include prenomination preferences in their empirical tests and the scholars examining the effect of prenomination preferences neglected to develop time-sensitive reciprocal models. Our goal is to develop a more comprehensive model of vote choice in presidential elections by merging these two trends.

A Revised Model of Vote Choice

In Figure 1 we diagram a dynamic simultaneous model of vote choice that includes a prenomination preference term. The model borrows liberally from those developed earlier (Markus 1982; Page and Jones 1979). Indeed, aside from the prenomination preference term all the variables appear regularly in contemporary models of vote choice. And the rationale behind the reciprocal paths linking these standard variables has been delineated by a variety of scholars (Fiorina 1981; Markus and Converse 1979; Page and Jones 1979). Even the placement of the actual vote term parallels earlier models. As Markus and Converse (1979, 1057) contend, policy evaluations, personal qualities, and party identification are not linked directly to the vote, but instead "their confluence yields a set of overall candidate evaluations, on the basis of which a choice is made." All that remains is to justify the theoretical ties between prenomination preferences and the other variables in the model.

After careful consideration of the proper causal placement of prenomination preferences in a voter's decision-making process we concluded that preferences were quite possibly simultaneously related to all the other variables in the model. For instance, it seemed quite likely that prenomination preferences could influence policy evaluations and that evaluations could influence preferences. Page and Jones (1979) found such a reciprocal link between general election preferences and policy evaluations, and we see no reason why prenomination preferences and evaluations would not be causally intertwined as well. Likewise, prenomination preferences may help shape individ-

Figure 1. A Revised Model of Vote Choice


Prenomination Preferences
Party Identification
Policy Evaluations
Comparative Personal Qualities
Comparative Candidate Evaluations
Vote

uals' views of candidates' personal qualities, and perceptions of personal qualities could influence preferences. After all, Markus (1982) determined that general election preferences and candidate qualities were simultaneously related. Prenomination preferences and party identification may also be caught in this causal web. Individuals' party attachments may strengthen or weaken depending on how their preferred candidate fared in the nomination struggle (Fiorina 1981), and the strength of individuals' party ties may influence their choice for the nomination (Abramson, Aldrich, and Rohde 1986; Bartels 1987). Finally, prenomination candidate preferences and general election preferences—termed *comparative candidate evaluations* in our model—could well be knotted causally. Certainly, prenomination preferences could influence general election preferences (Stone 1986). At the same time, though, opinions of the general election candidates may alter how individuals view candidates that were involved in the nomination contest. In all, then, much like general election candidate evaluations, prenomination preferences may well be related simultaneously to all other important variables influencing vote choice. This myriad of reciprocal paths is incorporated in Figure 1.

In addition to specifying the probable causal links between variables, we also render Figure 1 dynamic through the self-contained, looped paths associated with the variables. Each loop signifies that that variable is in part a product of past values on the variable (Markus 1982). For example, the prenomination preferences loop means that current preferences are in part a result of past preferences.

Data and Methodology

We employed the 1980 NES panel data to test our model outlined in Figure 1. The panel study, representing one phase of the 1980 NES project, sampled the opinions of a set of respondents at four time points during the election year: P1 in January; P2 in June; P3 in September; and P4 after the November election. The P3 (September) survey served as our primary data base because it tapped respondent feelings after the party conventions but prior to the November election. The P4 (November) wave was unusable because it did not include the feeling thermometer, issue, or candidate quality questions necessary for variable construction. Moreover, it measured respondent opinion after the general election. The P2 (June) survey was rejected because it assessed opinion at the culmination of the bitter prenomination season, long before the general election campaign had had the opportunity to mend intraparty squabbles.

The causal paths in Figure 1 were estimated by party and then reestimated using a more comprehensive sample of respondents. The party-specific analyses allow us to compare the decision-making processes used by Democrats and Republicans in determining their choice for president in 1980. The full sample analysis provides a more generalizable portrait of the interplay of forces structuring candidate selection.

We now turn to operationalizing the variables in Figure 1, beginning with comparative candidate evaluations. Following the lead of Page and Jones (1979) and Markus and Converse (1979), we conceptualized comparative candidate evaluations as the arithmetic difference between the scores respondents gave the two major party general election candidates, Carter and Reagan, on the standard NES feeling thermometer scales (Carter score minus Reagan score).

Among the determinants of comparative candidate evaluations is party identification. The standard NES seven-category party identification question was used to operationalize partisanship for empirical testing. On the basis of recent research showing that weak and leaning partisans behave similarly (Keith et al. 1986), we collapsed these categories into one. In addition, because the initial part of our analysis requires that respondents be segmented by party, the party identification question served to divide our analysis into Democratic and Republican constituencies. Only those respondents who identified themselves as Democrats or independents were included in the Democratic analysis, while the Republican analysis was comprised of the same independents and all Republicans. The independents were included in both analyses because their nonaffiliation prohibited us from knowing, a priori, which parties' prenomination battles should more strongly influence their general election preferences.

The issue orientations of a respondent may also influence general election preferences (Markus and Converse 1979; Page and Jones 1979). To test this possibility an issue variable was constructed that summarized respondent opinion on two of the most salient issues in the 1980 campaign: Carter's handling of both the Iran hostage affair and inflation (Markus 1982). Individuals were asked how strongly they approved or disapproved of the president's performance in these two areas. We simply summed these responses to form a policy evaluation score for each interviewee. Thus the final issue score for a respondent reveals how strongly he or she approved or disapproved of Carter's handling of two pivotal issues in the campaign.

In addition to partisanship and issues, respondents' perceptions of the personal qualities of the general election candidates help shape comparative candidate evaluations (Markus 1982; Markus and Converse 1979). Scholars have used the NES questions asking respondents how well the words and phrases *moral, dishonest, weak, knowledgeable, power hungry, inspiring,* and *provides strong leadership* describe the major party candidates to construct a comparative personal qualities index. The variable is calculated by summing each respondent's score for each candidate across the seven personal qualities and then figuring the arithmetic difference between the two candidates' scores. The resulting summary score quantifies which candidate's personal qualities the respondent most admires and by how much. We use this procedure to construct a comparative personal qualities index for Carter and Reagan.

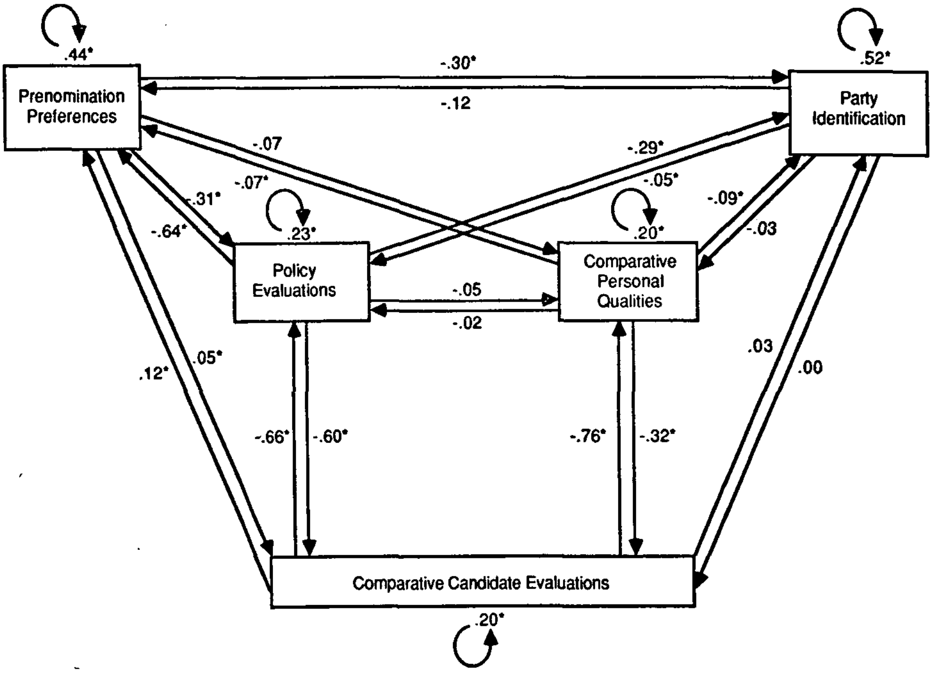
Party identification, policy evaluations, and comparative personal qualities comprise the variables traditionally used to account for vote choice. To this trio we add a measure of prenomination preferences. Our measure, patterned after comparative candidate evaluations, concurrently assesses respondents' preferences for the two major candidates seeking each party's nomination. For respondents in the Democratic analysis the variable was formed by subtracting the feeling thermometer scores given Kennedy from the scores given Carter. The prenomination variable for the Republican analysis was figured by subtracting respondents' scores for Bush from their scores for Reagan. The feeling thermometer scores for Kennedy and Bush were employed instead of those for the many of other unsuccessful candidates for the nominations (e.g., Anderson, Brown, and Connelly) because these two candidates were the principal challengers to Carter and Reagan after the first couple of months of the primary season. For the interparty analysis Democrat and Republican respondents (including leaners) were pooled. The preference variable was then refigured to form a continuum running from ardent Reagan Republicans to strong Carter Democrats. Specifically, Republican respondents' feeling thermometer scores for Reagan were subtracted from the scores for Bush and 100 was subtracted from the difference. For Democrats, Carter's feeling thermometer scores were subtracted from Kennedy scores and 100 was added to the difference. The result is a variable ranging from −200 for Republican respondents who scored Reagan 100 and Bush 0, to 200 for Democratic respondents who scored Carter 100 and Kennedy 0. Pure independents were excluded because we had no ready means of determining which party's candidates to use for variable construction (Stone 1986).

Estimating the causal paths in our revised model of vote choice is possible now that the variables have been operationalized. As stated above, we employed data from the September (P3) wave of the panel. The simultaneous nature of the model requires using two-stage, least squares regression (Markus and Converse 1979; Page and Jones 1979). When each of the variables was estimated to correct for any reciprocal bias, one of the predictor variables was the same variable from the January (P1) wave. Including the lagged variable in the simultaneous equations assesses the impact of previous voter opinion on current opinion, thus building in the dynamic nature of the vote choice process (Markus 1982).

Intraparty Analyses

The path estimates for the Democratic and Republican equations are presented in Figures 2 and 3, respectively. The estimates are standardized, two-stage, least squares coefficients. All the coefficients are in the expected direction except for (1) those between policy evaluations and comparative personal qualities in the Democratic model and (2) the path

Figure 4. Interparty Analysis.


.44*
-.30*
.52*
Prenomination Preferences
-.12
Party Identification
-.07
-.29*
-.07*
-.05*
.31*
-.09*
-.64*
.23*
.20*
-.03
Policy Evaluations
Comparative Personal Qualities
-.05
-.02
.03
.12*
.05*
.00
-.66*
-.60*
-.76*
-.32*
Comparative Candidate Evaluations
.20*

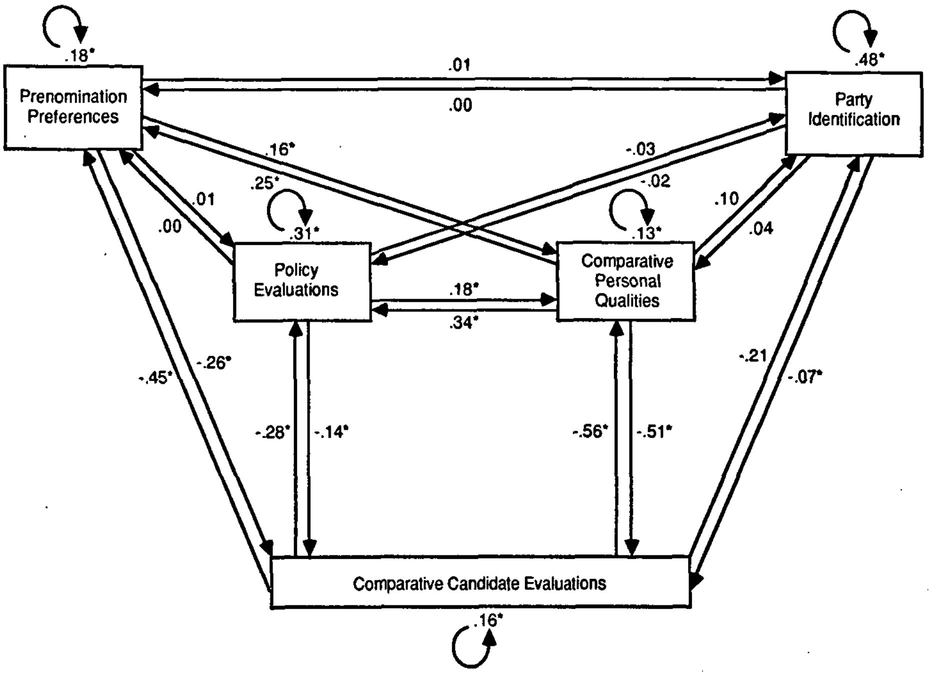
Note: N = 655; all entries are two-stage, least squares standardized coefficients.
*$p < .05$.

estimate running from prenomination preferences to party identification in the Democratic model.

Examination of the paths flowing from the prenomination preference variables show preferences having a strong direct effect on comparative candidate evaluations as well as an important indirect influence. Focusing first on the Democratic figure (Figure 2), we see that the coefficient associated with the path from prenomination preferences to comparative candidate evaluations is significant, indicating that the higher a respondent ranked Kennedy compared to Carter on the feeling thermometers, the more likely he or she was to rate Reagan high relative to Carter.

Figure 2 also suggests that prenomination preferences have an indirect influence on comparative candidate evaluations through policy evaluations. The healthy beta between preferences and policy evaluations implies that a portion of respondents who ranked Kennedy high compared to Carter vented their frustrations by criticizing the president's handling of the Iran hostage crisis and inflation. And the strong beta between policy evaluations and comparative candidate evaluations indicates that once critical of Carter's performance, many of these in-

Figure 3. Republican Analysis

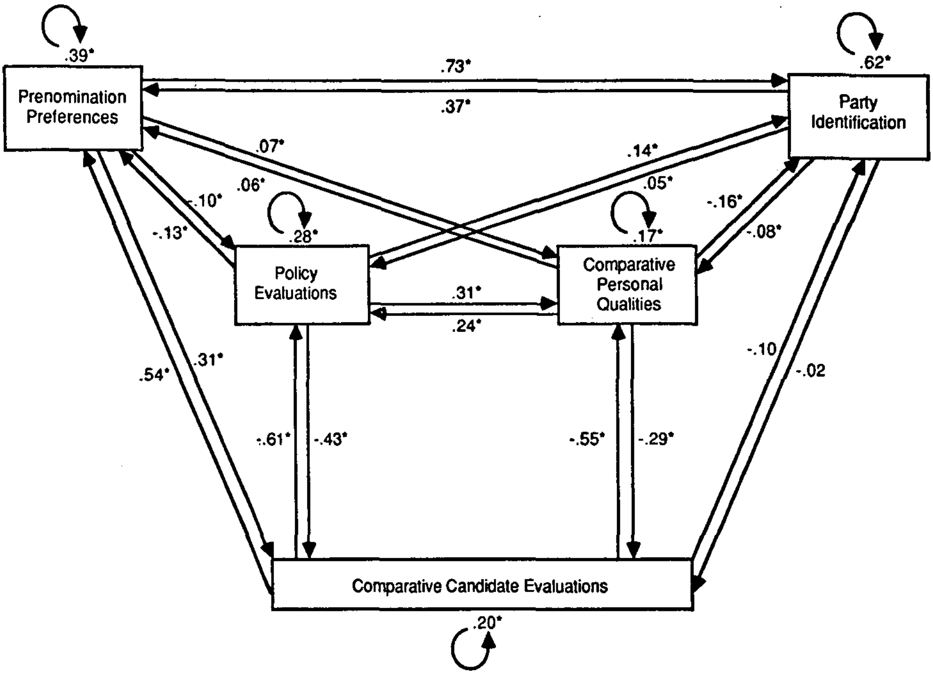

.18*
Prenomination Preferences
.01
.00
.48*
Party Identification
.16*
.25*
-.03
-.02
.01
.00
.31*
Policy Evaluations
.13*
Comparative Personal Qualities
.10
.04
.18*
.34*
-.45*
-.26*
-.28*
-.14*
-.56*
-.51*
-.21
-.07*
Comparative Candidate Evaluations
.16*

Note: $N = 490$; all entries are two-stage, least squares standardized coefficients.
$^*p < .05$.

dividuals proceeded to score Reagan near or above Carter on the feeling thermometers. The indirect effect of prenomination preferences on comparative candidate evaluations through policy evaluations is .19 (−.31 × −.60), almost four times the influence of preferences on general election candidate evaluations directly. It would appear, then, that a good number of disenchanted Democrats and independents viewed the election as a retrospective evaluation of Carter's presidency (Abramson, Aldrich, and Rohde 1983).

The results of the Republican analysis suggest that prenomination preferences played a more prominent role in the vote decision-making process among Republicans in 1980 than among Democrats. Figure 3 reports a robust coefficient of −.26 for the causal path leading directly from prenomination preferences to comparative candidate evaluations, over five times the magnitude of the same path in the Democratic model. The healthy coefficient implies that the more a respondent preferred Bush to Reagan, the more likely he or she was to score Carter high relative to Reagan. The coefficient may be stronger than its Democratic counterpart because most Republicans perceived Bush as more moderate than Reagan while

Figure 4. Interparty Analysis



.39*
Prenomination Preferences
.73*
.37*
.62*
Party Identification
.07*
.14*
.06*
.05*
-.10*
-.13*
.28*
Policy Evaluations
-.16*
-.08*
.17*
Comparative Personal Qualities
.31*
.24*
.54*
.31*
-.61*
-.43*
-.55*
-.29*
-.10
-.02
Comparative Candidate Evaluations
.20*

Note: $N = 842$; all entries are two-stage, least squares standardized coefficients.
$^*p < .05$.

Democrats quite often saw Kennedy as more liberal than Carter. Thus Republicans who preferred Bush to Reagan may have found it more acceptable ideologically to support the opposing party's nominee than did Democrats who preferred Kennedy to Carter.

Figure 3 also reveals that prenomination preferences had an important indirect effect on comparative candidate evaluations through personal qualities (.16 × −.51 = −.08). This indirect influence suggests that the higher a respondent ranked Bush compared to Reagan, the more likely he or she was to evaluate Carter's personal qualities more highly than Reagan's. In turn, the higher a respondent ranked Carter's qualities relative to Reagan's, the more likely he or she was to rank Carter near or above Reagan on the general election feeling thermometers.

Interparty Analysis

For a final test of our revised model of vote choice we estimated the paths with a combined sample of Democrats and Republicans. The estimates are presented in Figure 4. Once again, the vast majority of the coefficients are in the anticipated direction, the only exceptions being the betas associated with the causal links between party identification and comparative personal qualities and between party identification and comparative candidate evaluations and the path estimate running from party identification to policy evaluations.

The coefficient accompanying the causal arrow flowing from prenomination preferences to comparative candidate evaluations indicates that preferences had a strong direct influence on respondents' evaluations of Carter and Reagan. Indeed, the beta compares favorably with the coefficients running from policy evaluations and comparative personal qualities to comparative candidate evaluations and eclipses the path estimate running from party identification to evaluations.

Also of interest in Figure 4 is the sizable coefficient associated with the path from preferences to party identification. The magnitude of the beta lends support to recent work contending that party identification is susceptible to campaign-specific forces (Fiorina 1981), in this case prenomination preferences. The coefficient implies that Carter backers tended to move toward the Democratic end of the party identification scale during the nomination campaign and that Reagan supporters moved toward the strong Republican ranks. Kennedy and Bush supporters, meanwhile, may have weakened their party ties, with Kennedy backers moving away from the Democratic end of the party identification scale and Bush loyalists moving away from the Republican end of the scale. Evidently, the ramifications of a divisive prenomination struggle may go well beyond the general election. The primary battles may serve to weaken a party's standing among the electorate for the long term.

Lastly, it is pleasing to note that the comparative candidate evaluations variable in Figure 4—which was constructed as part of the two-stage, least squares estimation process—proved a trusty predictor of respondents' actual general election vote choices. To figure the forecasting utility of the evaluations variable we dichotomized vote choice (Carter = 1; Reagan = 0; all other candidates and nonvoters = missing) and employed logistic regression. When the comparative candidate evaluations variable was regressed on the dichotomized vote measure, the resulting coefficient was .043 with a standard error of .005 ($p<.001$). Although interpretation of the coefficient is not straightforward because logistic regression produces a logarithmic odds function, it is possible to calculate the change in probability of voting for one candidate over the other for a unit change in the evaluations variable (Petersen 1985). For our equation the calculation produced a tidy conclusion: a positive change in the comparative candidate evaluations variable of 1% corresponded to a 1% greater chance of voting for Carter.

As far as prediction is concerned, if we assume that respondents vote for the candidate they ranked highest on the comparative candidate evaluations variable, our model predicts 89% of the cases correctly—an impressive performance, given that the evaluations variable was constructed from September data. The Markus and Converse (1979) model, using November data and adding the predictive ability of party identification, predicted 90% of the cases correctly for 1972 and 97% for 1976.

Taken together, the three dynamic simultaneous analyses provide solid evidence that prenomination preferences played an integral role in shaping respondents' general election evaluations of Carter and Reagan. Results showed that even after controlling for a variety of other forces, preferences remained strongly linked to evaluations.

Discussion

Despite widespread agreement that vote choice is the result of a dynamic simultaneous process and equally widespread agreement that a prenomination campaign influences general election vote choice, researchers have failed to meld these two important trends. We employed the 1980 NES panel data to accomplish this task. A variable tapping respondents' prenomination preferences was fitted into a fully specified dynamic simultaneous model of the vote. When tested, the analyses substantiated the importance of prenomination preferences in the formation of general election vote choice. Indeed, the findings provide perhaps the most compelling evidence to date in support of the carryover hypothesis. Specifically, the results revealed that the higher a respondent scored Kennedy compared to Carter on the feeling thermometer scales, the more likely he or she was to rank Reagan near or above Carter on the general election thermometers. Likewise, the higher a respondent ranked Bush relative to Reagan, the more likely he or she was to rate Carter high compared to Reagan.

The impressive performance of the *prenomination preference* variable does more than simply supply evidence in support of the carryover hypothesis, however. It confirms prenomination preferences as a vital determinant of general election vote choice. To be sure, the precise effect of prenomination preferences will vary from campaign to campaign, but our findings suggest strongly that the prenomination season does shape individuals' attitudes toward the candidates and that these attitudes do carry over to influence the general election vote decision. Scholars modeling vote choice, then, should take care to include prenomination preferences in their equations. Indeed, models of vote choice that fail to consider prenomination preferences may well be specified improperly; thus their parameter estimates should be viewed with the utmost caution.

Note

The data analyzed were made available through the Inter-University Consortium for Political and Social Research, University of Michigan.

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SIMPLE EXPLANATIONS OF TURNOUT DECLINE

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Several recent studies have claimed to explain the post-1960 decline in U.S. presidential and congressional election turnout in terms of just a few variables. Abramson and Aldrich (1982) attribute the great bulk of the decline to aggregate declines in partisanship and political efficacy. Shaffer (1981) attributes still more of the decline to aggregate decreases in these same two variables plus age and following the campaign in the newspapers. And Kleppner (1982) attributes "virtually all" of the decline to aggregate decreases in the first three of these variables. We show how these studies measure explanation too generously and how the very short-handedness of their models makes them seem more successful than they are. These few variables, we conclude, leave most of the decline unexplained.

Among the most widely noted trends in U.S. political behavior is the disquieting decline in presidential and congressional election turnout. At the moment the curve may be bottoming out—the percentage voting edged up fractionally from 1980 to 1984—but from 1960 to 1980 it fell steadily. In the 1960 presidential election, 64% of those eligible voted; by 1980, the figure had fallen to 53%. Although the mobilization of blacks and women reversed the trend in southern states, the decline outside the South was correspondingly greater (from 71% to 54%).[1] Within the South turnout dropped among white males (Cassel 1979).

That turnout should have declined during this period is, as Brody (1978) has aptly termed it, a "puzzle." The most obviously relevant changes in the causal background—increased levels of education and the relaxation of registration requirements—logically should have *increased* turnout. Yet proportionally fewer and fewer people have been voting. Other more powerful variables must be keeping people from the polls. But what?

Several recent studies have provided engagingly simple answers. Abramson and Aldrich (1982) point to aggregate decreases in strength of partisanship and external political efficacy (perceived government responsiveness). These two variables alone, by their estimate, account for some 70% of the 1960–80 decline. Shaffer (1981) finds that decreases in age and following the campaign in the newspapers, along with decreases in partisanship and efficacy, explain 86% of the 1960–76 decline. Kleppner (1982, 130) concludes that the decreases in partisanship, efficacy, and age explain "virtually all" of the 1960–80 decline.[2]

These two-, three-, and four-variable explanations are beguiling, and Abramson and Aldrich's, in particular, is widely cited; but they are *too* simple. Partisanship, efficacy, and age have almost certainly contributed to turnout decline but are not the whole story. Much fuller accounts leave most of the decline unexplained (Ashenfelter and Kelley 1975; Cassel and Hill 1981; Reiter 1979); these slenderer ones can only appear to do better.

One source of this illusion is that these studies score explanation too generously, but a more basic source is precisely that their variable lists are too short.[3] The exclusion of variables that have acted to increase turnout exaggerates the explanation of net decline. The exclusion of other variables that have acted to decrease turnout allows the included variables to take credit for explanation that is not theirs. Even the exclusion of variables that have neither increased nor decreased turnout but do affect individual turnout cross-sectionally can produce a similar misallocation, depending on the measure of explanation.[4]

We shall concentrate here on these specification issues. After briefly indicating how these studies' statistics stretch explanation, we consider and refute Abramson and Aldrich's and Shaffer's claims of immunity from misspecification bias. To illustrate the point we show how adding variables to the Abramson and Aldrich model reduces the role of partisanship and efficacy. Although we confine the analysis to presidential elections, the argument applies equally to congressional elections and for that matter to other time series "explained" by underspecified models. The conclusion in the present case is that Brody's puzzle has been only partly solved.

Measures of Explanation

Let us first see how these studies reach their results. Abramson and Aldrich base their claims partly on "algebraic projections" of what turnout would have been if partisanship and efficacy had not weakened. To see how this works, let the proportion of the electorate in the i^{th} category of partisanship and j^{th} category of efficacy in the t^{th} election be s_{ijt} (with $t = 0,1, \ldots, 5$ representing 1960, 1964, . . . , 1980), the proportion of the people in this combination of categories who vote in the t^{th} election be r_{ijt}, and the t^{th} election turnout be T_t, so that

$$T_t = \sum_i \sum_j r_{ijt} s_{ijt}. \qquad (1)$$

Playing on this identity Abramson and Aldrich estimate (1) the proportion of the electorate that would have voted in the t^{th} election had the joint distribution of partisanship and efficacy remained what it was in 1960 as

$$\hat{T}_t = \sum_i \sum_j r_{ijt} s_{ij0}, \qquad (2)$$

(2) the decline that would have occurred under this null scenario as $T_0 - \hat{T}_t$, and (3) the proportion of the actual decline $T_0 - T_t$ explained by partisanship and efficacy as

$$\begin{aligned} A_1 &= 1 - [(T_0 - \hat{T}_t)/(T_0 - T_t)] \\ &= (\hat{T}_t - T_t)/(T_0 - T_t). \qquad (3) \end{aligned}$$

Shaffer and Kleppner form similar projections from each variable separately, substituting adjusted for actual turnouts within categories as a form of remedial partialing. To obtain the adjusted turnout for the j^{th} category of the k^{th} explanatory variable—say, the pure independent category of partisanship—in the t^{th} election, they regress individual turnout on the other explanatory variables.[5] Since the residuals reflect the extent to which the probability of voting is higher or lower than it should be on the basis of the other variables, Shaffer and Kleppner define the adjusted turnout for pure independents r^{*}_{jkt} as the overall turnout T_t plus the mean residual within that category. Then they define (1) the proportion of the electorate that would have voted in the t^{th} election if the distribution of partisanship had remained what it was in 1960 as

$$\hat{T}_{kt} = \sum_{j} r^{*}_{jkt} s_{jk0}, \tag{4}$$

(2) partisanship's contribution to the decline as $\hat{T}_{kt} - T_t$, and (3) the proportion of the decline all the variables together explain as

$$SK = \sum_{k} (\hat{T}_{kt} - T_t)/(T_0 - T_t).^{6}$$

This exhausts the relevant portions of Shaffer and Kleppner, but Abramson and Aldrich try a second, more sophisticated analysis, based on a probit model of individual turnout as a function of partisanship and efficacy. They estimate the probit parameters on the pooled cross-sectional data from the 1960–80 NES election studies, then combine the estimates with the t^{th} election means of partisanship and efficacy to generate the predicted turnout

$$\hat{T}_t = \phi(\hat{\alpha}_0 + \hat{\alpha}_1 \bar{P}_t + \hat{\alpha}_2 \bar{E}_t), \tag{5}$$

where ϕ is the cumulative standard normal density function, the $\hat{\alpha}$s are the parameter estimates, and $\bar{P}_t$ and $\bar{E}_t$ are the means of partisanship and efficacy. Then, finally, they regress the actual turnout T_t on this prediction, obtaining an estimate $\hat{b}$ of the slope b. Since the change in actual turnout that can be expected from a $\Delta\hat{T}_t$ percentage point change in the probit prediction, according to this linear regression, is $b\Delta\hat{T}_t$, and the proportion of the actual change ascribable to the predicted change is thus $\Delta\hat{T}_t/b\Delta\hat{T}_t = 1/b$, they set $A_2 = 1/\hat{b}$.[7]

On the surface these measures seem plausible enough, and they certainly produce rosy figures. By A_1, partisanship and efficacy explain 72% of the 1960–80 decline; by A_2, they explain 67%. By Shaffer's version of SK, partisanship, efficacy, age, and following the campaign in the newspapers explain 86% of the 1960–76 decline; by Kleppner's, partisanship, efficacy, and age explain 107% of the 1960–80 decline.

As this last, impossibly large number suggests, however, these measures run high.[8] SK is particularly inflated, because variable-by-variable projections attribute some of the same explanation to more than one variable, and the adjusted turnouts do not eliminate the overlap.[9] But A_1 and A_2 are inflated, too. They, too, as we show in Appendix A, can exceed 1.0. These are not proportions of the decline explained in the same way as the conventional R-squared is the proportion of variance explained. An SK, A_1, or A_2 of .7 is less than 70% of what it could be.

Specification

The deeper problem, however, is underspecification. These analyses omit all the variables that have acted to increase turnout. They even omit some of the variables that have acted to decrease turnout. And they omit all the variables that affect individual turnout cross-sectionally (compare Ashenfelter and Kelley 1975; Cassel and Hill 1981; Conway 1981; Patterson and Caldeira 1983; Powell 1986; Reiter 1979; Wolfinger and Rosenstone 1980).[10]

Abramson and Aldrich and Shaffer concede some omissions but claim dispensations. A first claim, made by both, is that because they are interested only in explaining variation over time they can ignore variables that affect individual turnout but whose distributions have stayed roughly constant. A further claim, made only by Abramson and Aldrich, is that because they are interested specifically in explaining turnout *decline* they can also ignore any variable whose distribution has changed, but in a way that should have increased turnout.

The first claim has a plausible ring.

Intuitively, a constant cannot explain a variable. In both linear and probit models, variables that are really constants blend into the intercept. Variables that are only approximately constant may have some effect, but the bias from omitting them depends not only on their effects but also on their correlations with the included variables,[11] and anything that has varied only weakly over time can be only mildly related over time to partisanship, efficacy, age, and following the campaign in the newspapers, which have trended more strongly.

All this is true and is applicable to strictly time series devices like A_1. In this one analysis, Abramson and Aldrich can legitimately exclude variables that affect turnout but are roughly stationary over time. But they also exclude variables that affect turnout and *have* varied over time: education, concern over the election outcome, age, residential mobility, contacting by political parties, and registration and voting requirements.[12] These variables are correlated over time with partisanship, efficacy, and one another; and their exclusion biases A_1. Bringing them into the analysis would erode the effects of partisanship and efficacy. For A_1, then, the first claim, though valid, does not cover the exclusions.

For A_2 and *SK* the claim is invalid to begin with. These statistics incorporate individual-level estimations, from which only individual-level, not temporal, invariance can excuse omissions. The omission of variables that affect individual turnout—even variables that have been relatively constant over time—inflates the probit parameter estimates in Equation 5 and the projected turnouts from Equation 4. The biased estimates from an underspecified model may manage in some sense to "explain" the decline, but some unknown portion of the explanation is really due to the variables the model neglects.

The second claim is simply false. It is false even when we do not estimate parameters at the individual level, as in A_1. Suppose, to proceed by analogy, that your bank balance falls by $500 during a month in which you can recall making $400 of deposits and only $500 of withdrawals. How far do these known transactions account for the decline? Entirely, if we go by the ratio of withdrawals to net decrease, analogous to A_1 (or *SK*) confined to variables decreasing turnout. But this is obviously not right: $400 is missing. We need to include the deposits. The real explanation of the decrease is only $(500 - 400)/500 = .2$. In broadly similar fashion the exclusion of variables increasing turnout from the probit equation underlying A_2 makes the correspondence between actual and predicted turnout artificially close. This too "neglects the deposits." We think we have done better than we have at explaining why turnout declined if we ignore the forces increasing turnout.

Some Illustrations

As this discussion implies, we should be able to deflate these results by introducing (1) variables that have increased turnout over time, (2) other variables that have decreased it over time, or even (3) variables that have not affected it much (because they have not varied much) over time but that do affect individual turnout cross-sectionally. Let us see what happens to Abramson and Aldrich's results when we add one variable of each type, one at a time.

For additions of type 1, the main question is how much the augmented model explains. Simply adding some such variable and recalculating A_1 and A_2 should yield lower estimates. Explanation, by these measures, can decrease with additional variables. Additions of type 3, on the other hand, should not affect explanation, while those of type 2 should increase it. What should decrease is the explanation allotted to partisanship and efficacy, once the new variable has been taken into account. To gauge this we define the multiple-partial measures A_1^* and A_2^*, described in Appendix B.

The variables we shall try adding are

1. *Education.* Along, perhaps, with the effective enfranchisement of blacks, the most salient variable increasing turnout over this period is education. How much of "education's" effect is education's per se is unclear (Cassel 1987; Luskin 1987), but education is at least correlated with the participatory norms, political knowledge, and cognitive and social skills that lighten the costs, and increase the perceived benefits of, voting. Previous studies suggest that it has a powerful influence on turnout (Campbell et al. 1960; Powell 1986; Verba and Nie 1972; Wolfinger and Rosenstone 1980).

   What happens when we add education to Abramson and Aldrich's analysis?[13] We begin with A_1. To conserve cases within partisanship-efficacy-education combinations, we divide education only into "high" (high school graduate or better) and "low" (no high school degree) and collapse the categories of partisanship and efficacy, from 4 to 3 and 3 to 2, respectively.[14] With partisanship and efficacy collapsed in this fashion, $A_1 = .83$: partisanship and efficacy explain 83% of the 1960-80 decline. When we add education, however, $A_1 = .24$: partisanship, efficacy, *and* education explain only 24% of the decline. A_2 beats a similar, if milder, retreat. Replicating Abramson and Aldrich's analysis, we find $\hat{b} = 1.51$, and $A_2 = .66$, only trivially different from their numbers.[15] With the addition of education, however, $\hat{b} = 1.91$, and $A_2 =$ only .53. From this angle, too, we see how the exclusion of variables increasing turnout can exaggerate explanation.

   At the same time, one might suspect with Abramson and Aldrich (1984) that controlling for education would increase the role of partisanship and efficacy. But let us look at our multiple-partial measures and see. $A_1^* = .49$; $A_2^* = .60$. Even this variable increasing turnout cuts into the explanation assigned to partisanship and efficacy. More direct competition can bring still larger drops in A_1^* and A_2^*, as we shall see.

2. *Concern.* Caring which party wins the election ("concern") should logically stimulate voting (Campbell et al. 1960) and has declined over this period.[16] Although the reference to party suggests some overlap with partisanship (Abramson and Aldrich 1982; Cassel and Hill 1981), the correlation between the two is only about .35—low enough to show that they are far from the same variable but high enough that they compete for explanation.

   Controlling for concern, $A_1^* = .61$. The percentage of the decline ascribed to partisanship and efficacy decreases by over 20%. In the probit estimation, $A_2^* = .56$. The percentage of the decline explained by partisanship and efficacy drops by 10%.

3. *Interest.* People who are more interested in the campaign are more likely to vote, in much the same way as more ardent basketball fans are more likely to attend basketball games. But interest has been roughly stationary, winding up only slightly higher in 1980 than in 1960, and can therefore have made little contribution to turnout decline.[17]

   For this reason, controlling for interest hardly affects the strictly time series measure A_1: $A_1^* \approx A_1$.[18] A_2, however, incorporates individual-level estimates of the probit parameters, and the ex-

> clusion of variables that affect individual turnout biases the parameter estimates for partisanship and efficacy. With interest in the equation, these latter decrease, leaving $A_2^* =$ only .42. The inclusion of interest, despite its lack of trend, slices 25% from partisanship and efficacy's explanation of the decline.

Even 42% is a lot of explanation from only two variables, of course, but this is still too flattering an estimate. A_1^* and A_2^* are still generous measures, still capable of exceeding 1. Even when less than 1 individually, moreover, the A_1^* or A_2^* for partisanship-and-efficacy-controlled-for-interest and the A_1^* or A_2^* for interest-controlled-for-partisanship-and-efficacy can sum to more than 1. They do not uniquely apportion the explanation among predictors.

But above all we have been adding variables only one at a time. A fair number of other variables have acted to increase or decrease turnout since 1960 or affect individual turnout. A truly well specified model, in which partisanship and efficacy would face multiple competition, would award them a still smaller share of the explanation. They might still account for a significant chunk—we suspect in fact that they would—but it would be considerably smaller than 42% even by this generous measure.[19]

Conclusion

All models are misspecified, to be sure. We deliberately make small omissions, trading completeness for parsimony, and must always live with the possibility of having unwittingly made larger ones. But we do think it fair to insist that a model be specified up to a certain subdisciplinary standard. Variables that theoretically ought to affect turnout, that empirically seem to affect it, and that are correlated with other variables that affect it cannot simply be waved off.

Precisely what variables we are obligated to include depends on the level of abstraction. At a sufficiently general level a model with only a handful of variables —costs, benefits, and probability of affecting the outcome (as in Downs 1957; Riker and Ordeshook 1968)—may be perfectly adequate. But if we are interested in knowing what the relevant costs and benefits consist of and what affects the perceived probability of affecting the outcome, we must disaggregate these categories into variables like partisanship, efficacy, and age. At this more concrete level, however, these few variables are not enough.

The standards of specification also depend on the level of analysis. At the aggregate level a time series analysis may safely exclude variables that have not varied much over time. But even a time series analysis must be adequately specified at its level, and at the partisanship-and-efficacy level of abstraction this means adding variables like education, concern, age, mobility, and registration and voting requirements. That some of these variables may have increased rather than decreased turnout does not justify omitting them.

As a practical matter no decently specified time series model can be estimated directly. The variables we must include are too many, the elections for which we have attitudinal data too few. For the foreseeable future, we shall have to estimate at the individual level, then aggregate. This is statistically preferable in any case, since aggregation prior to estimation sacrifices some efficiency (Kmenta 1986, 369-70). But since timewise invariance does not justify individual-level exclusions, we shall have to work with longer equations.

Most of these criticisms stand as general cautions. It may be tempting, whenever the object is to explain some aggregate trend, to take at face value measures like the ones in these studies. It may be tempting to disregard variables that have countered the trend. And it may be tempting to ignore variables that have only a cross-sectional effect even when the estimation is partly at the individual level. These are all traps for the unwary.

In the present case we have shown how the explanation shrinks as we add variables that have acted to increase turnout and how the burden of explanation shifts away from partisanship and efficacy as we add other variables—even, depending on the measure, variables that have not varied much over time and therefore cannot have contributed much to the decline but that do affect individual turnout.

We do not doubt that partisanship, efficacy, and age have played a part in decreasing turnout. But it is a much less important part than these studies would have us believe. What accounts for the rest is unclear, although it is hard to believe that increases in residential mobility and decreases in concern over electoral outcomes have not made a significant contribution (see Cassel and Hill 1981; also Squire, Wolfinger, and Glass 1987). It may also be that the increasing number of hours the average citizen has spent watching television is involved.[20]

It may never be possible to sort all this out. Nor can we realistically expect to explain 100% of anything. But we should not delude ourselves that these simple explanations are sufficient. Most of the post-1960 decline in turnout is still unexplained, and we shall have to do some hard thinking, then cast a wider net, to explain substantially more.

Appendix A: Demonstration That A_1 and A_2 Can Exceed 1.0

A_1. It follows directly from Equations 1–3 that $A_1 > 1$ when

$$\sum_i \sum_j (r_{ijt} - r_{ij0}) s_{ij0} > 0,$$

that is, when the within-category turnout rates have generally increased, with the overall turnout decline coming from increases in the sizes of relatively low turnout categories. For example, let there be just one explanatory variable, with just two categories. Let the first category contain .4 of the electorate, voting at a rate of .2, at time 0 but swell to .8 of the electorate, voting at a rate of .3, at time t. Let the second category contain the remaining .6 of the electorate, voting at a rate of .8, at time 0 but correspondingly shrink to .2 of the electorate, voting at a rate of .9, at time t. Then $T_0 = .56$, $T_t = .42$, $\hat{T}_t = .66$, and $A_1 = 1.71$.

A_2. The ordinary least squares estimate $b = 1 + \mathrm{cov}(\hat{u}_t, \hat{T}_t)/\mathrm{var}(\hat{T}_t)$, where $\hat{T}_t$ and $\hat{u}_t$ denote the predicted score and residual from the regression of the actual turnout T_t on the probit-predicted turnout $\hat{T}_t$, and *var* and *cov* denote the sample variance and covariance (see Luskin 1984). Thus $A_2 > 1$ ($\hat{b} < 1$) when the covariance is negative, that is, when the regression tends to underpredict larger T_ts and overpredict smaller ones. For example, let there be a six-election series for which the turnouts are .60, .58, .56, .54, .52, and .50, and the $\hat{T}$s from Equation 5 are .59, .57, .56, .54, .53, and .51. Here $\hat{b} = .79$, and $A_2 = 1.27$.

Appendix B: A_1^* and A_2^*

Note first of all that

$$\begin{aligned} T_t &= \sum_i \sum_j \sum_k r_{ikt} s_{ijkt} \\ &= \sum_i \sum_j \sum_k r_{ijkt} s_{ijt.k} s_{kt} \\ &= \sum_i \sum_j \sum_k r_{ijkt} s_{kt.ij} s_{ijt}, \end{aligned}$$

where s_{ijkt} = the proportion of the electorate in the i^{th} category of partisanship,

j^{th} category of efficacy, and k^{th} category of some third variable z at the t^{th} election; r_{ijkt} = the proportion of the people in that combination of categories who vote in the t^{th} election; $s_{ijt.k}$ = the proportion of the k^{th} category of z that falls in the i^{th} category of partisanship and j^{th} category of efficacy; s_{kt} = the proportion of the electorate in the k^{th} category of z; $s_{kt.ij}$ = the proportion of those in the i^{th} category of partisanship and j^{th} category of efficacy who also fall in the k^{th} category of z; and s_{ijt} = the proportion of the electorate in the i^{th} category of partisanship and j^{th} category of efficacy.

To define A_1^*, then, we estimate the t^{th} election turnout—with the joint conditional distribution of partisanship and efficacy within categories of z remaining what it was in 1960 and the distribution of z changing as it did—as

$$\hat{T}_{t.k} = \sum_i \sum_j \sum_k r_{ijkt} s_{ij0.k} s_{kt},$$

and the t^{th} election turnout, with the conditional distribution of z remaining what it was in 1960 and the joint distribution of partisanship and efficacy changing as it did—as

$$\hat{T}_{t.ij} = \sum_i \sum_j \sum_k r_{ijkt} s_{k0.ij} s_{ijt}.$$

Hence we define

$$A_1^* \, (\hat{T}_{t.k} - T_t)/[(T_0 - \min(T_t, \hat{T}_{t.ij})],$$

where for a z increasing turnout the denominator now represents the gross decline—the larger decline that would have occurred were it not for the operation of z.

To define A_2^*, we estimate the probit model using the third variable z as well as partisanship and efficacy, then simply fix the mean of z at its value for $t = 0$ (1960), setting $\hat{T}_{t.z} = (\hat{\alpha}_0 + \hat{\alpha}_1 \bar{P}_t + \hat{\alpha}_2 \bar{E}_t + \hat{\alpha}_3 \bar{z}_0)$ = the t^{th} election turnout we should predict if partisanship and efficacy varied as they did while z remained at its 1960 level. Then we regress T_t on $\hat{T}_{t.k}$ rather than $\hat{T}_t$, obtaining the new slope estimate $\hat{b}_z$ and defining $A_2^* = 1/\hat{b}_z$.

Notes

An earlier version of this paper was presented at the 1986 annual meeting of the Midwest Political Science Association, Chicago. We are grateful to Paul Abramson, Stephen Bennett, John McAdams, Herbert Walzer, and Herbert Weisberg for comments.

1. Turnout figures from the U.S. Bureau of the Census 1961, 349, 360 and U.S. Bureau of the Census 1986, 254–55.

2. Shaffer and Kleppner incorporate some partial controls for variables increasing turnout but use only these variables decreasing turnout to explain turnout decline.

3. For an earlier objection on this latter ground see Hill and Cassel 1983; for a response see Abramson and Aldrich 1984.

4. *Turnout* means the proportion of the electorate voting in a given election. *Individual turnout* means whether or not the i^{th} respondent has voted in the t^{th} election. Turnout is thus the mean individual turnout.

5. Kleppner uses "multiple classification analysis," a form of regression in which the regressors are automatically broken into dummies; Shaffer treats the regressors as interval.

6. This, at any rate, is the logic. Neither Shaffer nor Kleppner makes quite this selection of variables for the regressions that generate the residuals. Shaffer uses education in the regressions but not the explanation (excluding education's negative contribution to the decline from the numerator of *SK*); Kleppner uses education, income, and sex in the regressions but not the explanation and uses partisanship and efficacy in the explanation but not the regressions.

7. Strictly speaking, A_1 and *SK* should carry a *t* subscript, because even for a given starting point they yield different results for each election. The explanation of the 1960–76 decline is not the same as the explanation of the 1960–80 decline, and so on. In this respect, as well as in its ability to accommodate larger numbers of explanatory variables, A_2 is a clear improvement on A_1 and *SK*.

8. One reason for this specific embarrassment, as Kleppner recognizes, is that he excludes partisanship and efficacy from all the regressions to construct the adjusted turnout rates. The result is that the effect of partisanship is not controlled for efficacy, the effect of efficacy is not controlled for partisanship, and the effect of age is not controlled for either efficacy or partisanship. But there are also more intrinsic reasons. Even when perfectly applied,

SK can exceed 1.0, as for that matter so can A_1 and A_2. See n. 9.

9. The residuals from the regression excluding partisanship contain more than just the effect of partisanship. They also contain the effects of all the variables these studies neglect, the measurement errors in both individual turnout and the explanatory variables the regression includes, and the olio of idiosyncratic influences in a pure disturbance term. The residuals from the regression excluding efficacy, reflecting the same neglected variables, measurement errors, and idiosyncratic factors, are partly the same.

10. The cross-sectional analyses in A_2 and SK suggest the magnitude of the omissions. Estimating a linear model that includes education as well as the four variables he says are responsible for the decline, Shaffer obtains a mean within-election R-squared of .15. Abramson and Aldrich do not report the R-squared analog for their final, trivariate probit model, but for the bivariate model containing only partisanship they report an R-squared analog "roughly equivalent to a simple 'correlation' of 0.25" (p. 508). Since R-squared is a squared correlation, the R-squared analog, apparently, is $(.25)^2 = .06$.

11. Although we shall write loosely of "bias," the real issue in much of what follows is consistency (converging in probability on the true parameter as $n \rightarrow \infty$), since asymptotic properties are all the probit estimates can claim anyway. This in no way affects the argument.

12. In confining their analysis to whites, Abramson and Aldrich reduce but do not eliminate the effects of registration and voting requirements.

13. We follow Abramson and Aldrich in using self-reported rather than validated vote (which both they and Shaffer report makes little difference) and confining the analysis to whites. One small departure from Abramson and Aldrich is that we exclude 18-to-20-year-olds (except from Georgia and Kentucky) from all our analyses, whereas Abramson and Aldrich exclude them only from their probit equations. This, too, makes little difference (personal communication from Paul Abramson). By our calculations the percentages of respondents who reported voting for president in 1960–80, are 83.4, 80.4, 77.6, 75.5, 75.1, and 74.0 (based on *ns* of 1,574; 1,263; 1,166; 1,847; 1,861; and 1,088). These figures also exclude respondents with missing values on partisanship, efficacy, and vote. The 1960 and 1976 data are weighted.

14. We break partisanship into the same set of categories into which Abramson and Aldrich collapse partisanship in their probit model: independents, weak partisans (including independent leaners), and strong partisans. We have also followed Abramson and Aldrich in coding efficacy, then combined the two lowest categories.

15. Abramson and Aldrich, in their Table 6, report $\hat{\alpha}_1 = .295$ and $\hat{\alpha}_2 = .374$—close to our values, but reversed. Since we arrive at the same $\hat{b}$, we suspect that Abramson and Aldrich's table has the labels turned around.

16. Concern is coded as follows. For 1960, 1964, and 1968, 1 = *care very much* or *pretty much*, and 0 = *pro-con–depends, care a little* or *some, don't care at all,* or *don't know*. For 1972, 1976, and 1980, 1 = *care a good deal,* and 0 = *don't care very much* or *don't know*. The means for the election years 1960–80 are .69, .65, .58, .61, .57, and .56.

17. We use the item that reads, "Some people don't pay much attention to the political campaigns. How about you, would you say that you were *very much interested, somewhat interested,* or *not much interested* in following the political campaigns this year?" With the responses coded (1,2,3) in order of ascending interest, the means for the election years 1960–80 are 2.57, 2.73, 2.56, 2.86, 2.58, and 2.82.

18. For this computation we dichotomize interest as high or low, again to conserve cases.

19. Controlling for education, concern, interest, and age simultaneously produces $A_2^* = .38$ (the algebraic method of A_1 and A_2^* is impracticable with so many variables), but these are still not all the relevant variables, and A_2^* is still too generous.

20. Shaffer's newspaper variable may be reflecting this.

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Forthcoming in March

The following articles, research notes, and controversies have been tentatively scheduled for publication in the March 1989 issue:

John H. Aldrich, John L. Sullivan, and Eugene Borgida. Foreign Affairs and Issues: Do Presidential Candidates Waltz before a Blind Audience?

Henry Bienen and Nicolas van de Walle. Time and Power in Africa.

Henry E. Brady and Stephen Ansolabehere. Rational Choice, Knowledge, and Preferences for Presidential Candidates.

Gary W. Cox and Michael C. Munger. Closeness, Expenditure, and Turnout in 1982 U.S. House Elections.

Kenneth R. Mladenka. Blacks and Hispanics in the Political System: Public Jobs, Minority Power, and Political Effectiveness in 1200 Cities.

Clifford Orwin. Piety, Justice, and the Necessities of War: Thucydides' Delian Debate.

Robert Powell. Crisis Stability in the Nuclear Age.

George Rabinowitz and Stuart Elaine Macdonald. A Directional Theory of Issue Voting.

Michael J. Rich. Distributive Politics and the Allocation of Federal Grants.

Steven B. Smith. The Concept of Right in Hegel's Political Philosophy.

Philip D. Stewart, Margaret G. Hermann, and Charles F. Hermann. Modeling the 1973 Soviet Decision to Support Egypt.

George Tsebelis. The Robinson Crusoe Fallacy: The Abuse of Probability in Political Analyses.

REVIEW ESSAY

SKEPTICAL STUDIES OF LANGUAGE, THE MEDIA, AND MASS CULTURE

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Ronald Reagan, the Movie, and Other Episodes in Political Demonology. By Michael Rogin. Berkeley: University of California Press, 1987. 366p. $25.00.

Contested Truths: Keywords in American Politics since Independence. By Daniel Rodgers. New York: Basic Books, 1987. 270p. $19.95.

Shaping Political Consciousness: The Language of Politics in America from McKinley to Reagan. By David Green. Ithaca: Cornell University Press, 1987. 277p. $29.95.

American Media and Mass Culture: Left Perspectives. Edited by Donald Lazere. Berkeley: University of California Press, 1987. 618p. $48.00 cloth, $15.95 paper.

Studies of political communication have taken two quite distinct paths, and the divergence is probably widening. On the one hand there are investigations of the kinds of language and symbols that currently attract political support and votes. This form of research is more pragmatic than theoretical. It relies on findings from surveys, voting statistics, and psychological experiments to reach conclusions about how votes, attitudes, and behavior can be changed or reinforced and has become a key influence in the planning of political campaigns, the marshaling of support for policies, and the shaping of postures toward social institutions, public and private. As far as many of their key uses are concerned, these studies yield knowledge in the service of power.

The other tendency is more ample in its theoretical reach and its historical depth and is likely to be critical and analytical respecting the foundations of power. It draws from a range of theoretical orientations of the last two centuries, notably language philosophy, psychoanalysis, literary theory, western Marxism, and poststructuralism.

The four books I discuss here fall into the second category. Though their subject matters are diverse, these studies complement one another regarding some important themes: language as a crucial form of political action; the political potency of some forms of expression that present themselves as entertainment, culture, science, or fantasy; the pervasiveness of contradiction as a device for indoctrination; and the uses of science and cultural forms as buttresses of established political power.

Political Demonology

In his latest book Michael Rogin continues the insightful and disturbing probing of U.S. political demonology that distinguished his earlier study of American Indians. His essays spring in part from imaginative applications of Freudian theory but more from Rogin's lively intelligence and his talent for recognizing latent meanings in movies and in language. Rather than accepting the common view in social science that the penchant for constructing blacks, Indians, political dissenters, women, or foreigners as demons is an aberration, an instance of political extremism, or a marginal political position, he sees demonology as fundamental to U.S. politics.

The research Rogin presents here calls for a formidable combination of knowledge, imagination, and logical rigor because it synthesizes aesthetic creations with conventional political action in ways that enhance understanding of both. While positivists will no doubt complain that the conclusions are not falsifiable, this book is marvelously enlightening about the wellsprings of U.S. political causes, passions, and leadership even if some of its specific conclusions are debatable. Rogin's work demonstrates again that it is precisely the ambiguities and diverse meanings of symbols that make them politically potent. It should encourage application of that insight to the ambiguities and diverse interpretations that characterize both falsifiable research conclusions and everyday political discourse.

A recurring theme is the inseparability of fantasies and symbols from substance and the related idea that beliefs about issues generally regarded as widely separated in content and meaning may be closely linked psychologically, metaphorically, or ideologically. While symbolic interactionist, phenomenological, Freudian, and poststructuralist theories have all suggested that perspective and applied it, I know of no work on U.S. politics that does so as provocatively as this one. When Rogin concludes that Reagan's film roles taught him who he was and documents the claim through an analysis of those roles, he is offering a striking instance of his conviction that the whole culture is "increasingly impervious to distinctions between fiction and history" (p. 9).

The adverb is probably unnecessary. The imperviousness, I suspect, is not so much a characteristic of late-twentieth-century U.S. culture as of how human beings "know" history. The pervasiveness of film, the electronic media, and literacy today doubtless makes the fantasies to which people are exposed more uniform, more banal, and more politically manipulable than they used to be, as Walter Benjamin recognized in his celebrated essay on reproducible works of art. But the construction of reality through analogies and narrative is unavoidable. Indeed, the pretense in mainstream science and in common political discourse that the distinction between fact and fiction is clear is itself a major source of illusion and ideology because it insulates the constructions labeled "fact" from criticism, constructions that are likely to reflect the currently dominant ideologies because they build on accepted ways of seeing. The view taken by Michael Rogin and Jean Beaudrillard that fantasy can be "hyperreal" is more tenable. The realities created by movies, television programs, and other art forms become key influences upon interpretations of reported and witnessed events. Discrimination among them cannot be based upon the simple distinction between fact and fiction.

Rogin regards the various demons in U.S. history (nonwhites, women, the Catholic Church, the USSR, "terrorists") as creations of a "countersubversive tradition." They have often served a common political function and substituted for one another as well as succeeded one another. Rogin sees the war in Vietnam as the culmination of a policy of suppression, intimidation, and control toward Indians and other nonwhites—an example of his talent for recovering what is repressed in our discourse. He might have added that intimidation of some groups is typically masked either by a vocabulary about "helping" those groups or by narratives about their progress toward freedom.

This kind of sociolinguistic and historical scholarship illuminates the ways in which liberal ideology has on occasion provided sets of symbols that rationalize repression. Rogin suggests that Indians living in a communal order and in harmony with nature threatened independent and lonely liberal men who saw such harmony as an impossible nostalgic dream. So Indians living the dream became a threat that had to be erased by killing Indians and making the survivors dependent on whites. The forced removal of Indians from their ancestral lands became a model bearing a resemblance to the forced removal of the poor in urban renewal programs and the forced urbanization of peasants in Vietnam. In an analogous sense he proposes that "the similarities between immigrants and Negroes initiated the reunion between North and South" (p. 195).

In an absorbing examination of the pioneering movie *The Birth of a Nation* Rogin interprets the film's portrayal of race war in the South as an evocation of the other social conflicts of the postbellum decades as well: class, ethnic, and sectional antipathies and the threat posed by the "new woman," whose sexuality was shifted in the film and in public anxieties from women to blacks in order to reempower the white man. There are comparable observations about transfers of meaning in a chapter on cold war films.

This kind of analysis breaks the boundaries between political issues conventionally defined as distinct from one another and also shows how texts about history and art forms presented as fiction construct one another. The exposition is an important contribution to political psychology. Like some work in semiotics and some in poststructuralist thought, it probes cognitive processes and political impulses that probably cannot be examined in any other way.

Rogin makes his contribution chiefly by the astuteness with which he assesses the meanings of specific phenomena rather than by a pretence of discovering general laws. While the phenomena fall under a small set of rubrics like demonology and the transfer of meanings among diverse issues, it is the specific analysis rather than any grand generalization that is valuable. In that sense the sum of the parts of this book is greater than the whole.

Language and Politics

Both Daniel Rodgers and David Green are historians whose books examine U.S. political history by focusing on terms that have appeared often in political discourse. Rodgers, the author of a widely praised earlier study of the work ethic in the United States, examines the entire period since independence, while Green's book focuses on the years from McKinley to Reagan. In many ways the two studies complement each other, though they examine the twentieth century through different key terms. Rodgers' foci *(utility, natural rights, the people, government, the state,* and *interests)* denote philosophical themes or labels for institutions, while Green's terms *(individualist, paternalist, progressive, reactionary, liberal, conservative,* and *anticommunist)* designate ideologies.

Both studies embrace the premise that language is itself a crucial form of political action. ("The bigger, the more sonorous the words, the more private desires they can bind together, the more new desires they can create," as Rodgers put it [p. 4]). Both emphasize the capacity of widely used labels to evoke diverse and contradictory meanings.

This last point is crucial to understanding both the power and the limitations of language as a form of political action. In considering it these scholars are applying an insight of the twentieth-century revolution in language philosophy. Words like these reify: they reduce the uncertainties, flux, and contradictions characteristic of abstract concepts and political maneuvers to a misleading clarity and a substantive denotation, at least for the moment and for the particular situation. But such problematic focusing and reifying does not displace the capacity of language to evoke a wide range of connotations, some of them contending with others and justifying diverse policies. Political language allows all the multivalence that characterizes groping for a stand on a controversial issue to express itself even as it rationalizes whatever specific course of action permits an individual to live with his or her situation. The social condition and the language define one another, a point repeatedly illustrated in these accounts. Both books illuminate the indispensable contribution historical analysis makes to the understanding of political language.

Rodgers and Green pay a great deal of attention to the language of political commentary, criticism, and analysis, and especially to the language of political scientists. They highlight some properties of language in general and of scientific language in particular that social scientists too often ignore at the cost of contaminating their studies.

Green calls attention to the risks of perversion of analysis inherent in the use of everyday political labels as descriptive or scientific terms rather than as objects of analysis in themselves. When we use words like *government, the people, liberal,* or *anticommunist* as if they were designations for discoverable entities or ideologies, we forget that the terms are themselves political weapons, *constructing* entities and ideologies rather than simply referring to them, and creating them differently as social contexts change. Their ability to construct realities is, of course, precisely what makes such terms useful to politicians, public officials, and interest groups. A social scientist who accepts them as references to an objective reality without also treating them as devices for reifying a set of dubious premises becomes a purveyor of a doctrine. Skepticism regarding language forms and criticism of their specific uses by political actors and political analysts is vital. It does not yield objectivity; but it does generate awareness that belief in the objectivity of a form of language is a lethal pitfall for researchers, as it is for anyone who discusses the political scene.

Rodgers offers a history of the language of U.S. political science. In doing so he calls attention to the ideological role that political science has played, and he scrutinizes some concepts that have helped keep the public acquiescent to governmental authority. After the Civil War professional political scientists and constitutional lawyers began to formalize amateur talk about politics, their intent and their effect being to set limits on the powers of the general public. They relied on abstractions unfamiliar to that public, especially "the state," a constructed entity that is also a nonentity. "If words are instruments," Rodgers writes, "then one of the clearest uses of the sovereignty-endowed state was to blunt every dissident claim of right outside those rights the State itself chose to bestow" (p. 172). While Green examines the ideological implications of a pretense that common terms are also scientific concepts, Rodgers emphasizes the elitism implicit in inventing uncommon and abstract terms that win wide currency. Both are right because both usages rationalize and reinforce prevailing biases.

Political scientists have changed their preferred concepts from time to time in ways that reflected currently fashionable fears and hopes and so buttressed them. The focus on "the state" faded around the turn of the century into a preoccupation with "interests" and with empiricism. These terms continued to justify governmental authority to cope with "parochial" or "special" interest groups and parties and generated suspicion of words connoting unobservable entities or wholes. But with World War II and the cold war that followed it, political science rediscovered the virtues of "the state," now detecting a wholesome unity created by the very divisions into overlapping interests that had previously been regarded with suspicion.

Though Rodgers does not mention it, the widespread focus on the term *political system* (and its offspring, *political socialization)* in the political science textbooks and journals in the 1960s and 1970s reinforced the same inclination to value established institutions and to define their maintenance as a major goal of politics and political science. If language is a political weapon, the language of social science has proven an especially effective one and never more so than when it claimed to be professional, analytic, and scientific.

The focus on key words in the books by Rodgers and Green is both an enrichment and an impoverishment. Historical analysis of the functions of common terms highlights some important phenomena associated with public support for political leaders and candidates that conventional methods miss, notably the short-circuiting of thought implicit in political labels, their capacity to condense conflicting anxieties and aspirations into a potent symbol, their ability to concentrate attention on some concerns and aspirations while masking others, and the leeway for inconsistency and the exercise of policy discretion that the "ownership" of a popular label confers upon political leaders and parties.

But a paramount or exclusive focus on key words implies that they explain the thrust of policy in the eras and the administrations that use them, and that view is certainly an exaggeration. Every controversial policy issue evokes its own distinctive language forms. A liberal or conservative label for a regime has only a tenuous bearing on support or opposition for its positions and actions on abortion, capital punishment, military action in the third world, or racism. Each such issue requires an exploration of the specific tropes, myths, scientific claims, and stories that create the issue and of the contradictions implicit in each of them. Analysis of the functions served by words that label major entities, ideologies, leaders, and parties sometimes becomes tedious because it deals with too little of the political scene and builds too superficial a historical account. It also can create a temptation to treat language as a magic force that is independent of the social and economic situation in which it is used, though these writers are aware of that pitfall and usually avoid it. In any case these books are valuable contributions that will influence future political studies by demonstrating that thoughtful scrutiny of language probes aspects of politics that more conventional research methods miss or misconstrue.

Media, Mass Culture, and Politics

The anthology edited by Donald Lazere brings together a wide range of critical writings on the media and on popular art and culture, including film, music, theater, sports, fairy tales, architecture, and pedagogy. They show that forms of culture and expression accepted as science, art, entertainment, or news are likely to be more powerful influences upon political postures and actions even than those that are explicitly political in their content and purpose. To define texts as "political" generates wariness about their ideological content; but works of art and culture diffuse ideology while evoking reactions that are likely to be attributed to their merit as art, science, or entertainment. That response creates a kind of political communication that is privileged, both because it benefits from its association with a valued cultural form and because it generates presuppositions that are likely to escape or minimize criticism of their political content. Though few of the contributors to this collection are political scientists (Stanley Aronowitz may be the only one), they enrich political studies by identifying latent political influences.

Donald Lazere's introductions to each of the eight parts of the volume do a great deal to shape such a healthy perspective. Lazere displays an encyclopedic knowledge of the literature on culture and the media and an impressive talent for evaluating its diverse and conflicting themes.

The subtitle of this collection proclaims that it offers "left perspectives"; Lazere notes that they are generally to the left of mainstream Democratic party positions. But it is more important that their posture is skeptical, inclined to question the accepted wisdom about the roles of the media, social institutions, and cultural trends. No ideology dominates in this collection, though the Frankfurt school has been a strong influence on many of the contributions, and these tend to reflect that school's disdain for popular culture. The anthology is nonetheless marked by striking diversity in approaches, ideologies, concerns, and aspirations, as well as some unevenness in quality.

An article by Todd Gitlin, "Domesticating Nature," cogently explores one of the paramount properties of political discourses—their attempt to compensate with symbols for the values that people are denied in their everyday lives. In the case Gitlin examines, the symbols are not words, but rather flora and fauna. The posh hotel in San Francisco that places plants, a stream, and birds in its vast lobby creates the impression that capitalist enterprise begets nature rather than destroying it and also discourages the nonwhites and the poor who inhabit the city from entering this artificial space and so shields its elite clientele from their presence and the feeling of guilt they might bring with them. Insulation, reassurance, and quiescence are certainly major political functions of cultural forms intended for elite audiences.

In another thoughtful paper Gitlin offers a sophisticated statement of how entertainment, culture, and news reporting are hegemonic in contemporary society. His thesis is that propagation of the dominant ideology is not so much an imposition as a collaboration. Influential communication incorporates discourse that challenges the prevailing order as well as justifying it, subtly inducing its audiences to cooperate in resolving the contradiction so as to let them live with their existing situations.

Some recent challenges to Gramsci and the concept of hegemony offer evidence that the disadvantaged are typically quite aware that they are being conned. Especially compelling studies that bear that message have come from James Scott (*Weapons of the Weak*) and Kristin Bumiller (*The Civil Rights Society*), both of whom see the acquiescence of the deprived as more tactical than ideological. Awareness that ideological influence rests upon contradictory premises that elicit the cooperation of the disadvantaged contributes provocatively to that debate.

A paper by Jack Zipes on classic and contemporary fairy tales exemplifies a form of analysis to which such art historians as Arnold Hauser and, more recently, Thomas Crow have made rich contributions—the uses of works of art as stimuli of the political imagination and as creators of political presuppositions. The Zipes article explores some fertile themes: the fairy tale as a reaction against the rationalization of modern life and its focus on technique; the power of the fantastic to help us experience and cope with social reality by uncovering the repressed in human evolution; the bearing of fantasy on the impulse to restructure society to make it livable; the efforts of some contemporary fairy tale writers to express both a struggle against authoritarianism and the limits and possibilities of individual aspirations so as to help their readers reflect upon the realization of a better society.

Many of the pieces in this collection point to issues and forms of scholarship that are likely to be defined as vital to political science in the next several decades because they ignore artificial and inhibiting boundaries between politics and the imaginative life of contemporary human beings, enlarging on one of Rogin's central themes.

Conclusion

These four studies and some others like them remind us that consciousness is not so much a physiological attribute as a changing product of the language forms and cultural institutions around us. In the twentieth century the construction of political consciousness has facilitated an unprecedented sequence of atrocities and policies that failed, while rationalizing the persistence of social inequalities. Franz Kafka may have been the century's most insightful political analyst. Though there continue to be challenges to these depressing developments and though we know that they rest on contradictory cognitions, studies like these offer little hope that the political patterns of the twenty-first century will be markedly different.

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POLITICAL THEORY

Idioms of Inquiry: Critique and Renewal in Political Science. Edited by Terence Ball. Albany: State University of New York, 1987. 225p. $44.50 cloth, $14.95 paper.

While there are several books that survey the "state of the discipline" of political science or address intrasubspecialty concerns such as What should political theory be now?—the purposes of this book are unique. The editor hopes to engage mainstream political scientists in "conversations" with political theorists practicing within a variety of "idioms" of inquiry. These idioms and authors include situational analysis (Farr), rational choice theories (Hardin), hermeneutics (Ball), Jurgen Habermas's critical theory (White), Foucault's genealogy (Gibbons), political structuration (Dallmayr), realism (Isaac) and feminist theory (Ferguson).

Like many political theorists, Ball is puzzled by a major anomaly in the discipline of political science—its inability to escape "the behavioral strait jacket" despite the fact that "no one —not even latter-day behavioralists—seems to be especially enthusiastic about this turn of events" (p. 3). Ball suggests that political scientists and political theorists (should?) share at least one common question—Why has political science not progressed beyond behavioralism? Addressing this question could be the beginning of new, more fruitful and ongoing conversations between political scientists and political theorists. In pursuit of such conversations, the contributors tried to structure their essays so that some of the major presuppositions and tenets of the idiom would be clear to a nonspecialist. They also test the limits and utility of each idiom by applying it to a consideration of one or more political or theoretical problem. While a few of the essays are exceptionally clear or provocative, I doubt that the volume will satisfy its wider objectives.

Part 1, together with the last essay by Moon, are the least satisfactory portions of the book. These essays focus on questions of the meanings of rationality and progress in political inquiry and the contributions theory can make to the practice of politics or political science. Ball argues that political scientists should adopt a variant of Imre Lakatos' philosophy of science. Scientific research is guided by a theoretical "core" protected by a "belt" of protective assumptions. In order for fruitful research to proceed, this core must often be left undisturbed and adjustments permitted in the belt. Ball argues that several promising research programs in political science, Marxism, for example, have been killed prematurely by "dogmatic falsificationists" (p. 34). Since there can be no research program and hence no intellectual progress without a theoretical core, it is in the interest of the entire discipline to nurture a variety of theories. However, Ball does not address the much more fundamental questions raised by Ferguson and Foucault as to the meaning of a will to science, of its connections to the will to power, and of the ambiguities of "progress" and the question whether political theorists should participate in shoring up a belief in the possibility or desirability of a "science" of politics.

These questions also apply to Farr's claim that we should reconceptualize political science as generating "rational" explanations rather than empirical "covering laws." This reconceptualization seems both thin and naive, given the problems with "reason" and the politics of knowledge raised by subsequent contributors. Hardin attempts to clarify different concepts of the "rational actors" in Arrow's and Neumann-Morgenstern's versions of rational choice theory. This essay is difficult to follow since its contrasting of the two theories does not successfully illuminate the basic premises of rational choice theories. Hardin did not provide a persuasive argument for more extended conversations with this idiom. He concludes that while game theories provide much "conceptual clarification," the "value of many of the most technical developments in directly resolving problems" is "limited" (p. 88). Even this hard-won "elegance" may have to be sacrificed if the theory's practical utility is to be increased.

The next section discusses interpretative and critical theoretical approaches. Ball argues that the actual practice of political science more resembles literary interpretation than laboratory science. Political scientists would produce better interpretations or be happier with what they already do if they would comprehend the true nature of their work. However, Ball does not adequately support either of these claims, nor does he explore the complexities of assigning *his* interpretation of the practices of political scientists to them. White commends Habermas' "communicative model" as an interpretation of modernity and as an alternative research program. It is better than a purely interpretive one because it creates a critical distance on interpretative moves within a particular game by establishing minimal rules about what is to count as conversation. Unlike positivism, Habermas' research program is self-consciously and explicitly normative and ties discourse directly to political practices. However, White does not successfully deal with the limitations of Habermas' neo-Kantian concepts of rationality and the subject upon which, as he acknowledges, the entire theory is built. Nor does he question what it means or on what basis theorists can or ought to help "to orient the self-understanding" of progressive political movements (p. 132).

These are precisely the questions the authors influenced by Foucault address. Gibbons constructs a clear contrast between interpretive approaches and Foucault's. Foucault forces us to confront the relations of power, truth, and knowledge. His "subject" is constituted (problematically) in and through disciplinary and constraining practices. Foucault also challenges the desire for unified theories including the desire of those who believe such theories will contribute to more emancipatory practices. Ferguson extends Foucault's analysis to gender-based relations of domination and the ways they pervade and structure the discourses of political science, especially its notions of power and the "rational subject." Unlike virtually every other writer in the book, she attempts to locate political science within the most enduring power-knowledge relations of contemporary society. This location is a source of some of the nontheoretical reasons for the continuing grip of behaviorism in the discipline. The provocative insights of her essay support Foucault's claims about "submerged discourses" and their power to disrupt dominant ones. But this same power impels the dominant discourses to ignore subjected ones —as mainstream political scientists and many political theorists continue to do in relation to feminist theories. The un-self-reflective discussions of "realism" by Isaacs or of the "contributions" Moon claims a "social science" can make to (undifferentiated) "modern society" seem ironic, unrealistic, and innocent in contrast to Ferguson's careful treatment of the ambiguities involved in attempts to generate or sustain nondominating discourses.

These essays are sometimes helpful in summarizing and evaluating the assumptions of important idioms of inquiry. Most of the essays are clearly written, concise, and contain a minimum of idiom-specific jargon. However, I wonder what mainstream political scientists would make of any or all of them. Perhaps there is a larger problem with the metaphor of conversation and its necessary correlate, the rational participant. It—like Habermas' theory —posits a "will to know" divorced from power. But if this is inaccurate, what are the implications for those who wish to change the subjects of conversation? This is an essential question suggested but not adequately acknowledged or addressed by most of the contributors to this volume.

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The Crisis of Liberal Democracy: A Straussian Perspective. Edited by Kenneth L. Deutsch and Walter Soffer. Albany: State University of New York Press, 1987. 304p. $44.50 cloth, $16.95 paper.

The legacy of Leo Strauss continues to inspire philosophical reexamination and political debate. Most of the papers collected in this volume of 15 essays were first presented at a 1983 conference on Strauss and liberal democracy held at State University of New York, Geneseo. Part 1 describes the central philosophical and political questions that occupied Strauss during his lifetime. Part 2 examines Strauss's teaching on liberalism. And the two essays in part 3 attempt to relate Straussian teachings to the U.S. political experience.

Those familiar with Strauss's writings will not be surprised to learn that liberal democracy—and its current "crisis"—has its roots in a quarrel between ancients and moderns. Modernity has lost its way concerning the issues that matter most to the moral character of civilization. Liberal democratic people have become neutral, noncommittal, or nihilistic regarding the nature of a good human life. This emphasis on the separation from permanent and absolute ethical standards is nicely captured by the editors when they write that "the crisis of liberal democracy is best understood as a crisis of moral foundations" (p. 1). Yet with the exception of contributions by Gunnell and Salkever, this Straussian axiom is never seriously examined or challenged.

Platt delivers an encomium to Strauss that both offers a glimpse of Strauss's apparent power as a teacher "of the brightest and most manly" (p. 24) and also serves to frame Strauss's thought in terms of three recurring problems: ancients and moderns, philosophy and religion, and philosophy and poetry. Gourevitch and Masters reconsider Strauss's teaching on natural right. Gourevitch provides a concise summary of Strauss's views of historicism, conventionalism, and natural right. He suggests that Strauss is more skeptical of the foundation for a narrow theory of natural right than is sometimes thought, a view that nonetheless is consistent with a belief in "a permanent framework of permanent problems that the unaided reason may not be able to solve once and for all" (p. 42). Masters offers a nifty analysis of evolutionary biology, natural right, and natural science. He contends that Strauss's view of the relationship between natural right and natural science suffers from a misunderstanding of natural science, the product of a misinterpretation of Aristotle's *Physics*. This misunderstanding of classical science is traced to the slighting of Aristotle's biology in favor of his physics. Masters argues that once the neglected place of biology is restored to its proper place in science, contemporary natural science can be seen as "not automatically hostile to a doctrine of natural rights" (p. 60).

Gunnell's essay, published previously in *Political Theory*, explores Strauss's views on liberal democracy in order to clarify the relationship between political theory and politics. To Gunnell, Strauss represents the tendency in academic political theory to remain aloof from politics, to speak about politics in a highly rhetorical and abstract idiom. This tendency is a sign of the alienation of political theory from political life.

Part 2 includes essays by Cox on liberality in Aristotle and Machiavelli; Berns on freedom and equality in Aristotle and the moderns; Best on Lockean consent; Pangle on nihilism, democracy, and Nietzsche; and Eden on why Weber was not a nihilist. In a timely exchange due to the popularity of Bloom's recent criticisms of higher education, Gildin and Gourevitch debate the intersections between Strauss's conceptions of vulgar and philosophic virtue, liberal education, and liberal democracy. Gildin's Strauss thinks that classical political philosophy and modern constitutional democracy are in principle compatible as long as the quality of liberal education is properly maintained. Gourevitch sees more tension in Strauss's account of the fit between the classical virtues and liberal democracy, suggesting that Strauss gave the "gentlemen" a more prominent place than Gildin acknowledges.

In the volume's third part, Salkever offers a revision of liberal democracy that owes more to practical thinkers like Aristotle, Tocqueville, and Madison than to the more "abstract" theories of early modern liberalism. Salkever's excellent analysis casts doubts on the merits of considering modern liberal democracy as in "crisis." A better and more critical understanding of contemporary liberal democracy would see the regime's possibilities for the enrichment and preservation of democratic traditions. Salkever takes history more seriously than did Strauss, and he is therefore led to pay closer attention to actual political virtues found in U.S. political experience. The formulation of liberal democracy that emerges from this analysis is less high-strung than the Straussian "crisis" alternative and thus skillfully avoids the tendency to "understand ourselves in the misleading light of a contrast between some Nietzschean or Marxian nightmare of a radically privatized present and a dream of a republican community of virtuous citizens" (p. 263).

Bluhm reviews the contribution of social choice theory to democracy. Bluhm believes that the deepest problems of politics will not be successfully addressed by focusing on the *summation* of preferences but demand instead study of the *formation* of preferences.

The volume's sectarian tone results from a focus on the thought of Leo Strauss and on Strauss's writings on liberal democracy rather than on more catholic understandings of contemporary democratic theory and practice. Indeed, readers seeking to deepen their understanding of Strauss's political theory have the most to gain from examining this volume. Others less interested in Strauss or Straussian debates will find less reward.

GREGORY LEYH

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The Self-organizing Polity: An Epistemological Analysis of Political Life. By Laurent Dobuzinskis. Boulder: Westview, 1987. 226p. $37.50.

This is an ambitious book. The author promises to provide an account of cybernetics that will be both free of the defects of systems analysis and sufficiently powerful to redirect the focus of political inquiry, to transcend the chasm between positive and normative political theory and to rescue politics from social determinism and idealistic voluntarism. In order to accomplish this transformation of the subject matter and the methodology of contemporary political science, Dobuzinskis harnesses theorems from biology and thermodynamics to theories of continental thinkers like Morin, Piaget, and Habermas. The result is a conception of political life depicted in terms of the complexity of self-generating systems situated in changing environments and confronting dialectical tensions between autonomy and evolutionary adaptations.

In addition to tracing the development of cybernetics and its deployment in the natural and social sciences, the first three chapters attempt to demonstrate the superiority of "second-generation" models of cybernetics over earlier conceptions and to defend the utility of "new" cybernetics as a research hypothesis for political inquiry. Citing its excessive preoccupation with equilibrium maintenance and its technocratic commitment to an ideal of centralized and rationalized social control, Dobuzinskis acknowledges that there are good reasons to repudiate systems analysis. But he suggests that the problems with systems analysis are related to an impoverished conception of "negative feedback," which limits a system's response to external disturbances to the restoration of the status quo ante. In contrast to restrictive homeostasis, second-generation cybernetics emphasizes "positive feedback," a homeorhetic response that accelerates internal changes in order to ensure the stability of an autonomous formative process. In other words, homeorhetic response involves a process of "autopoiesis," or "self-production." In response to external pressures, the complex system possesses a capacity to maintain its integrity by transforming itself, producing structures and processes that did not exist before.

According to Dobuzinskis, the great virtue of the new cybernetics lies in the recognition of such autonomy. In affording a holistic approach giving substantive content to the notion of organized complexity, cybernetics provides an account of the "logical and even physical prerequisites to the knowledge and practice of self-production"; and in so doing "it helps us to comprehend freedom as a concrete reality" (p. 42). Moreover, as the "science of control," cybernetics refocuses political inquiry on power relations that are too frequently lost in market models of politics; it restores a balance between concern with the system and interest in the singular event; and it sensitizes analysts to the irreducible uncertainty introduced into political life by the autonomy of self-generating systems.

Through a wide-ranging discussion of autopoiesis in organisms as diverse as cells and stars, Dobuzinskis suggests that his cybernetic model may also provide a more sophisticated understanding of evolution, for "biological evolution, systemic differentiation and growth are isomorphic processes answering to an overarching logic, namely the ontogeny (i.e., individual development) of autopoietic unities" (p. 73). Dobuzinskis suggests that a consideration of the epistemological implications of this isomorphism culminates in recognition that there is a human telos. Whether understood in terms of individuals or of the species, the human telos is linked to the progressive development of cognitive and moral capabilities identified by Piaget and Habermas.

Dobuzinskis suggests that the evolutionary potential of the autopoietic polity is manifested in a "national psyche" that generates and legitimates the structures of the nation-state. As self-producing systems, nation-states create not only laws, institutions, and procedures to adapt to environmental disturbances but also the internal and external crises that warrant subsequent responses. They create the conditions for their own transformation and, perhaps, transcendence. Dobuzinskis cautions that "to analyze the evolution of the species in terms of a developmental logic that includes a moral dimension does not imply that history moves towards the triumph of good over evil. It simply means that we can—but will not necessarily—become more and more conscious of, and act upon, the hierarchy of values sustaining political life" (p. 98).

If evolutionary cybernetics does not imply an irreversible logic of history, it does nonetheless suggest specific tasks for politicians and political theorists. Arguing that legitimate state action must be conceived in terms of the production and reproduction of national identity, Dobuzinskis suggests that politicians must "cross-examine" the national community, affording opportunities to reexamine values and to confront the implications of conceptions of the good incorporated in existing institutions and policies. Similarly, the task of political theory is "to confront actors not only with the unintended, but also the unavowed consequence of their action, to challenge their motivation and to assess the significance of the norms that are applied to the evaluation of available options" (p. 196). Utilizing empirical, hermeneutic, and psychoanalytic methods, political theorists may thereby enable political actors to realize the evolutionary ethical imperative of "fraternalization" and to transform their nations accordingly.

The Self-organizing Polity touches upon a number of important issues. It recognizes that positivist commitments preclude analysis of important political questions pertaining to human nature, the relation between individual identity and collective identifications, and the role of indeterminacy and autonomy in situations of choice. It recognizes that a new methodology for the discipline must be nonreductionist, sensitive to issues of language and interpretation, sophisticated in its treatment of the interpenetration of the normative and the empirical, and attuned to history in examining claims concerning invariance and change. It recognizes the importance of preserving a conception of truth that sustains distinctions between ideology and science. It recognizes that the natural sciences may still afford metaphors of enormous heuristic value for the social sciences.

Yet the treatment of these issues is far from satisfactory. Dobuzinskis develops a cumbersome jargon that is more obfuscatory than illuminating. He recommends a tolerance for paradox even in cases where apparent contradictions could be easily dispelled by more rigorous analysis. He endorses a combination of positivist and postpositivist analytic methods without resolving their fundamental incompatibility. He offers an evolutionary teleology of politics that rests upon a number of conceptual errors. And perhaps most importantly, he offers a defense of the new cybernetics that dismisses rather than refutes some of the most telling criticisms of the model and hence affords no real reason to return to a defective systems approach.

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Critical Social Science. By Brian Fay. Ithaca: Cornell University Press, 1987. 242p. $35.00 cloth, $12.95 paper.

Brian Fay's *Critical Social Science* is a clearly written, lucidly argued, and remarkably different book on the problems and contributions of critical social analysis. Despite its title, which indicates a book oriented to the philosophy of social science, Fay does not focus on well-worn epistemological and methodological issues such as whether or not a social theory can be scientific and critical at the same time. Rather, he has written one of the most complete accounts of the *philosophy* behind a broad range of critical, scientific analyses of social practices and structures, as well as many social movements from Marxism to feminism. Fay's central point is that all forms of critical social science properly conceived share a common set of deep, underlying assumptions that both ground their endeavors philosophically and give a direction to their calls for social transformation. Such a philosophical analysis yields insights into the self-understanding of all social critics who aim at human emancipation. It is also useful for teaching this type of philosophy and social science, as well as for reflecting on the successes and failures of projects of social change in modernity. The book is written from a sympathetic yet critical perspective and assesses both the strengths and weaknesses of the ideals and values of emancipatory theory and practice. Whether one agrees with Fay's conclusions or not, it is clear that critical social science cannot do without this type of reflection. Fay has successfully revealed the sometimes unexamined presuppositions of many of the "theories capable of transforming human life."

Fay begins his book with a fairly uncontroversial set of requirements, which delimit the type of social science that interests him. In light of this definition, Fay draws the more controversial inference that many prominent critical theorists—like Marcuse and Adorno—are to be excluded. The purpose of Fay's strict definition of the term *critical social science* is to give a description of critical theory in its most complete and adequate form. To qualify, critical social science must meet four criteria: it must be scientific in the usual sense; critical with regard to the present state of society; practical, hence oriented to changing the circumstances it finds so defective; and, finally, nonidealist, in that it does not make ideas alone sufficient for social change (p. 26). Adorno and Marcuse are neither sufficiently scientific nor really practical (p. 4); Nietzsche's genealogies are not oriented to change or enlightenment (p. 33). One could also use these criteria to show that Foucault is also, in many important ways, not a critical theorist. By giving this set of necessary and sufficient conditions of critical social science, Fay then hopes to be able to elucidate the "ontology" required to make this type of enterprise possible. Fay means by *ontology* something like a philosophical anthropology, "a basic view of existence."

What is this basic view of existence in critical social science? Fay argues that the enterprise must be based in a specific, fairly detailed set of beliefs, which he calls "the humanist version of the self-estrangement theory" or "the activist conception of human beings" (p. 17). This ontology becomes what it is in modernity —an anthropology and not a theology or metaphysics. This is the same anthropology that is present in Charles Taylor's recent work. It sees human beings as self-interpreting, self-creating creatures with certain dispositions and capacities. Correspondingly, society must be basically artifactual and conventional. It is not a fixed or natural order but one capable of being radically changed and transformed. Both imply an ethics. In anthropology it is an ethics centering on the value of self-clarity. The theory of society suggests the value of collective autonomy. Fay then uses this ontology to arrive at a "basic scheme" of all critical social science, that is, a number of theories and subtheories that elucidate the present human conditions of suffering and frustration, their dependence on "false consciousness," and the political means and potential audience available to engage in self-creative, free activity to change it.

Taken at a very general level, I find this type of analysis quite fruitful. It does seem that a theory of change "ontologically" presupposes a being capable of change, and so on. But once Fay begins to specify the specific assumptions of this supposed shared "ontology" of all critical social science, I have my doubts. It seems doubtful that everything Fay includes in the ontology is a necessary presupposition of critical theory. As Quine points out, theories underdetermine their ontological commitments, so that Fay seems to give only one of many possible, empirically equivalent "ontologies" of critical social science. This underdetermination of all ontologies suggests that Fay has perhaps taken the wrong approach if he wants to get at the deeper underlying assumptions of critical theory. What is needed is not an "ontological," but a transcendental, analysis of the conditions of possibility for social criticism and social change (if that is possible). While Fay certainly indicates a number of capacities and facts about human beings presupposed by critical theory, he needs to distinguish more clearly between necessary and empirical conditions of possibility. He often confuses them, since they are both "ontological" in his sense. Thus, for all the insights Fay's ontological approach generates, particularly concerning the excessive assertions about human powers, I do not think he succeeds in identifying either fundamental, necessary assumptions or basic, nonalternative values that all critical social science inevitably shares.

Perhaps the most challenging and important part of the book is Fay's analysis of the limits of critical social science: the ontology of critical social science gives it the pretension of executing unlimited possibilities of rational change, self-clarity and collective autonomy. Fay is right to criticize some of the overly Kantian, rationalist emphases of most critical social science. Unexamined, large-scale assumptions about the human species should be eliminated and replaced by modest, empirically specific ones. Here hermeneutics and neo-Aristotelianism might supply much-needed correctives, for they give insight into the finite, embodied, historical situation of human speakers and actors. However, they are corrective only as long as we do not adopt their equally problematic a priori ontologies of finitude, which Fay finds "inherent" in reason, meaning, and action and which make critical social science impossible. Here again I am troubled by just this a priori status Fay gives to the limits he identifies as part of the human condition. Sometimes it seems that Fay takes them to be inherent, hence insuperable, limits. Fay's discussion never answers a crucial question: How can we tell an "inherent" from a historical limit? Do these limits really entail that clarity or autonomy are not worthy goals, particularly on a more modest scale? Values of critical social science retain their importance even if we recognize trade-offs between them and a plurality of other social values and goals. Autonomy and self-clarity need to be important values in themselves. Otherwise the trade-offs between them and happiness would really not be as tragic as Fay suggests they are. In the end Fay really shows the essential tensions of these values with others, not their inadequacy or limits.

For Fay, the analysis of these limits serves an important heuristic purpose: the upshot of the book is that the inadequacies of these "impossible" ideals demands a rethinking of the ontology of critical social science. This program may be desirable for a complete critical social science, and Fay goes some way in carrying it out. But it is so not because of inherent, but because of historically relative limits on reason and freedom. Like Gadamer, Fay often seems to be confusing historical and ontological limits. The relation to tradition is a historical question of relative dependence or independence, not an ontological question. As Marx put it, man makes history, but he does not make it as he pleases. This remark, and many others like it, already do what Fay finds so lacking in critical social science: they relativize some of the one-sidedness and bad effects of their ideals through self-critical reflection, not by changing ontologies. Moreover, the absence of any reference to the long tradition of critical social science's reflection on its own activities, on their own limits and bad effects, is a major weakness in Fay's book. Rather than really changing an underlying ontology, it is more appropriate to characterize what Fay is doing as bringing the values of critical theory into tension with aspects of historical existence.

All in all, Fay's book is an important contribution to critical social science, particularly as critical social science has come to question its "grand theories" like historical materialism, as well as its "grand projects" and "narratives" like infinite progress and complete emancipation. His analysis points in the direction of correctives needed to overcome the one-sidedness and exaggerations of much critical social science. These correctives suggest a less grand, more modest, and fallibilistic vision of the tasks of criticism and change. Fay's book succeeds when it helps us appreciate the general truth of Kant's insight into the justification of claims to knowledge: that the best way to defend a rational enterprise is to see its limits clearly and to point out its excesses. Fay ultimately provides just such a powerful defense of critical social science.

JAMES BOHMAN

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Lives, Liberties, and the Public Good: New Essays in Political Theory. Edited by George Feaver and Frederick Rosen. New York: St. Martin's Press, 1987. 272p. $29.95.

This is a collection of eleven essays offered in tribute to, and by colleagues of, Maurice Cranston upon his retirement from the chair of political science at the London School of Economics. In addition to the essays the book contains a complete bibliography of Cranston's works and a postscript by Cranston recounting his intellectual journey, his disposition toward Lockean liberalism, and his long-standing concern with the problem of liberty in the modern world. It is this problem that provides the integrating theme for these original essays, most of which are of very high quality, with even the least satisfying of them having something interesting to say about liberty. Also, given Cranston's devotion to the life and works of John Locke, it is not surprising to find that most of the essays speak with a decidedly Lockean accent.

Two of the better essays in the collection are on Hobbes. The essay by Mansfield is an attempt to appreciate the influence of Hobbes on modern notions of the role of the executive power in a republican government. Mansfield's main point is to show how Hobbes, despite his insistence on absolute sovereignty, prepared the way for an acceptance of an executive power that was less than absolute. Mansfield argues convincingly that Hobbes' sovereign is less than absolute, in part due to "Hobbes's own contribution to his sovereign and on what his sovereign owes [both] to him" and to others to whom he must turn for counsel (p. 31). That no philosopher of the modern executive power, from Locke on past Hamilton, read Hobbes in this nuanced way casts doubt on Mansfield's historical claim that Hobbes influenced the modern accommodation to a strong yet limited executive. Nevertheless, the strength of Mansfield's essay is in teasing out of Hobbes the latent weaknesses of a sovereign presented as absolute.

Orr's essay on Hobbes follows nicely upon Mansfield's in exploring the validity of Hobbes' claim to have offered to prospective sovereigns the first true science of politics. Orr offers several examples where Hobbes deviates from pure geometric demonstration in his arguments, concluding that "he gave us more of a miscellaneous polemic than he cared to admit" (p. 52).

The best—and longest—essay in the collection is Wokler's "Rousseau's Two Concepts of Liberty." With the only essay showing a pronounced anti-Lockean flavor, Wokler does a masterful job defending Rousseau against those who read him as a friend of totalitarianism. He accomplishes this through a lucid display of Rousseau's multidimensional understanding of liberty. Wokler makes it clear that the general will emerges from independent-minded, introspective, and free individuals. For Rousseau, "in order to be a citizen of a *res publica* one must look deep within oneself for a personal commitment to a collective goal" (p. 86). Wokler's essay should be read by anyone seriously interested in Rousseau.

In one of the most interesting essays, Kenneth Minogue explores the degree of loyalty a liberal state positively requires of its citizens. After considering factors that have diminished the power and dignity of the modern liberal state, Minogue concludes that "the liberal state must be held together by a process of negotiation involving all sorts of groups" (p. 223). The object of the public's passion for the liberal state must be its "procedure for dealing with disagreements," or "the rules of the game" (p. 223).

William Letwin's essay is a tame discourse on the exercise of discretionary judgment in constitutional government. He considers the limits that constrain the judgments made by public administrators, Supreme Court justices, and constitution makers. Letwin's discussion is clear, employs interesting examples, and would be especially useful in undergraduate courses on law for its perspective on the nature of a constitution and of public officials' responsibility to a constitution.

A more complete treatment of law and its relation to liberty is contained in Shirley Robin Letwin's essay. She provides a compact but remarkably insightful historical overview of the philosophy of law, showing how the ancient ideal of the rule of law has, to our great misfortune, come to be seen as the enemy of justice, and how "The history of the idea of law in modern times may be read as an odyssey in search of a way to restore a rational moral element to law without returning to the classical cosmos" (p. 237). Letwin shows how moderns like Kant, Savigny, Marx, legal positivists, critical legalists, and Dworkin have failed to provide this rational moral element, and argues for a return to "government by a stable system of rules" that would articulate principles of moral virtue.

Lakoff, in speculating on the influence of Rousseau on Tocqueville, explores the different ways a cautious historian and a speculative philosopher approach the topics of liberty, equality, and democracy. Rosen treads on some fairly well worn ground in an essay on Bentham's influence on J. S. Mill, with a specific focus on "the extent of Mill's indebtedness to Bentham for the underlying structure of *On Liberty*" (p. 121). Rosen argues that it is in the connection each makes between security and liberty that we see Mill's dependence on Bentham most clearly.

Feaver offers a psychobiographical study of the personal relationship of John Stuart to Harriet Taylor Mill and that of Sidney Webb to Beatrice Potter Webb, attempting to show connections in each case between their political ideas and the quality of their relationship. He makes an interesting case that Mill came to defend individual liberty because of his sensitivity toward society's disapproval of his marriage. But—as is typical of this approach—the general argument is all too neat, and one's instinct is to resist facile claims like "Sidney's disappointment in love had helped to destroy whatever lingering hold the theory of social and political individualism might otherwise have had on him" (p. 156).

Essays by Cotta and Polin each employ problematic philosophical abstractions to make what are in essence conservative attacks on Marxism and on modernity. Both are provocative, yet unsatisfying.

I would recommend this collection to anyone interested in some fine new essays on liberalism and to scholars interested in the writings of Maurice Cranston.

PHILIP ZAMPINI

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Democratic Education. By Amy Gutmann. Princeton: Princeton University Press, 1987. 321p. $19.95.

This is more than just another contribution to the genre of philosophy and public affairs. *Democratic Education* is exemplary. Political theorists ritually acknowledge the theme of education as critical, but they seldom tackle it. By virtue of its subject alone this book is important. From an analytical perspective, Gutmann's arguments defending democratic control of education against rival claims are scrupulous and ultimately compelling. The things we want from education are various and complex, she insists, pointing up the inadequacies of conservatives who are exclusively concerned with protecting the rights of parents against the state and of liberals who are so preoccupied with educating people to be autonomous and impartial that they risk disillusionment with morality. The organization of the book is very tight. Chapter by chapter Gutmann chips away at these rival positions, exposing their limitations step by careful step.

From a policy perspective, the book delivers advice and invites controversy on a host of subjects: vouchers and private schools, federal regulations, student decision making, teaching of creationism, sex education. The list goes on.

But what makes this study exemplary is that analytic arguments and policy proposals are in the service of an overarching political vision, that authority over education should be democratic because the purpose of education is conscious social reproduction, which in the United States means the reproduction of democratic citizens.

Gutmann derives constraints on democratic authority over education not from liberal premises but from democracy itself. Because social reproduction should be conscious, education must be nonrepressive. Because education aims at reproducing universal citizenship, it must be nondiscriminatory. Majority rule can be constrained to ensure democratic sovereignty in the future. She also considers securities. For example, educational professionals are a check on democratic authority and at the same time the most dependable source of democratic curricula. In contrast to other professional groups whose claims of expertise insolently challenge democracy, teachers strengthen it. For this reason they should be granted more of the rewards of professionalism, including merit pay and greater autonomy in such matters as graduation requirements and working conditions.

Within the limits of nonrepression and nondiscrimination, Gutmann charges democratic authority with deciding most things. The trouble with book banning is not only that it is repressive (in fact, it is not always repressive) but also that it is done arbitrarily. The same is true of the actual practice of textbook control by states. The latitude she allows democratic authority emerges most clearly when it comes to distributive justice. The principle of nondiscrimination prevents exclusion from primary education, but beyond establishing a threshold it does not say how much should be spent on education, who it should be spent on, or how children should be distributed within and among schools. These are for democratic processes to decide in light of people's understanding of what citizenship requires and their willingness to pay. "Equal opportunity" offers little guidance here. It could mean equalization (but at what level and at what price to overcome special disadvantages?), meritocracy (communities might not want to justify any inequalities above the threshold), or the implausible principle of maximization. Gutmann would give citizens discretion. Moreover, she looks at the structures of authority that would make democratic control effective.

Thus in a section on financing schools she shows that local communities will not be sufficiently empowered unless citizens decide not only how much to spend on education but also how this relates to spending on other goods. Local referenda on school budgets register only the desire to slow spending and taxation, not how to budget resources overall. Paradoxically, community control of schools turns into an argument for participation in financial and distributive decisions at the state, and often at the federal, level. Where some contemporary democratic theorists evoke "community" rhetorically, Gutmann is concrete.

Communitarian theorists today also want to move beyond liberal individualism by showing that community "penetrates the self." But typically their discussions are vague about the processes by which character is formed, values internalized, and habits engendered. Gutmann points realistically to the several contexts in which personality develops, setting schooling alongside (and sometimes against) parents. She measures education's limits vis-à-vis other formative forces, like television. She shows that if the kind of self we want is one genuinely identified with citizenship, then education contributes certain things more reliably and efficiently than other institutions. What kind of pedagogy creates democratic identity? Gutmann offers an account of the "morality of association" as it operates in schools, a morality based less on principles than on habitual interactions. This discussion could have been expanded. It is one of several in which she exploits the psychological lessons of conservatism in the service of democracy.

The author sets boundaries to her subject, saying only as much about democratic theory as she must to defend democratic authority over education against alternative views. As a result, her picture of citizenship is sketchy and must be culled from her examples. They suggest that Gutmann is wonderfully attuned to the diversity of political processes, to pluralism and complexity. But this political insight poses a problem for her theory of education. Once we are alert to the various ways in which democratic authority is exercised, it is not enough to say that education should produce citizens with the capacity for deliberation and social commitment. For participatory democracy is not of a piece: adversarial democracy calls up different attributes and skills than consensus building; electoral politics requires different qualities from both voters and candidates than does representation on planning boards or school committees. How important are powers of speech and persuasion relative to negotiating or organizational skills? The point is that "deliberation" is not one thing, and from a pedagogical perspective the various demands of participation may be incompatible even if politically we need them all. *Democratic Education* succeeds in its contest with liberal and conservative theorists over who should control education and why. It calls for a complementary account of democratic citizenship.

NANCY ROSENBLUM

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Rousseau's Exemplary Life: The "Confessions" As Political Philosophy. By Christopher Kelly. Ithaca: Cornell University Press, 1987. 262p. $29.95.

It has been said that for the study of Aristotle's philosophy it is important to know the following details about his life: he was born, he spent his life in philosophizing, and he died. Aristotle himself, one senses, would have heartily endorsed the sentiment.

Rousseau, evidently, did not. For he left us his *Confessions*, his longest book, in which he reveals in far greater detail than any philosopher before or since the most personal and at times excruciatingly intimate facts of his life. He tells us, for example, not only of his early life as a vagabond, a lackey, and a music teacher, of his transformation into the most celebrated philosopher in all of Europe, and of his later decline into a restless fugitive fleeing persecution both real and imagined but also of his preference for spanking, of his erotic relations with a woman he called "mama," and of his desire to expose himself. The question arises, Why did Rousseau write this book?

Despite Rousseau's own assertion that the work "will always be a precious book for philosophers," the prevailing tendency in the interpretive literature has been to understand the *Confessions* as itself a product of some of the more bizarre psychological traits depicted within it: it is just Rousseau exposing himself again; or it is the desperate attempt by a victim of persecution and paranoia to vindicate himself against his enemies. (A particularly interesting exception to this tendency is the recent *Modern Self in Rousseau's Confessions* by Ann Hartle.)

Christopher Kelly's superb new book on the *Confessions*, while not altogether dismissing such psychological factors, reveals and elaborates in a convincing manner the philosophically serious meaning of this strange work. He argues that it must be understood in terms of three distinct although interrelated purposes.

First, the *Confessions* is meant to be a treatise on human nature. Through this intimate history of the development and corruption of his own character, Rousseau is providing the necessary supplement to the hypothetical history of the species given in the *Second Discourse* and the imaginary history of the individual given in *Emile*—all of these "histories" serving the purpose of demonstrating Rousseau's new theory of man's natural goodness (and thus refuting the theory of original sin classically defended in the *Confessions* of St. Augustine) by displaying concretely how all human vices are acquired over time from society. Furthermore, just as the *Social Contract* and *Emile* go on to show how the state and the ordinary individual (respectively) can be restored to goodness if guided correctly by an extraordinary individual (the lawgiver and the tutor), so the *Confessions* goes on to explore the necessary question of how the extraordinary individual might cure himself.

On the second level the *Confessions* is an epistemological treatise. The truth about human nature, according to Rousseau's well-known theory, has been hidden from us by the many layers of artificiality and corruption we have acquired in society. And because reason itself is not natural to man, the progress of this faculty, so far from leading us back to self-knowledge, has only deepened our alienation from nature and ourselves. Now, if Rousseau is correct in this theory, it is incumbent on him to explain how it was possible for him, for any civilized human, to have discovered it. In the system of a thinker like Aristotle, who maintains that the truth is by nature and in principle available to all men, the details of his own life are of no philosophical significance. But Rousseau must write an epistemological autobiography, much as Hegel writes the *Phenomenology*, in order to explain how access to the truth became possible through him.

Finally, the *Confessions*, like all of the major writings of Rousseau, also has a practical, reformist purpose. The corrupt peoples of old Europe, whom Rousseau considers too enervated ever to be formed into public-spirited citizens, could still be given a somewhat healthier existence if they could be induced to withdraw still further from public life, withdraw into the narrower, less-corrupting circle of intimate friendship and domesticity. But how does one promote such reform? Most men live their lives by example, molding their tastes and actions to the pattern of certain "exemplary lives." Rousseau thus endeavored to reform men through his personal example, by living a life of conspicuous withdrawal. And he sought to extend the influence of his example by writing his *Confessions*, which by its form—autobiography—as well as by its content would confer new dignity on self-absorption and impress upon men the idea that the private, subjective, inner world of feelings and imagination has more reality than the corrupting public world of honor and status.

Kelly's book is a detailed analysis of the *Confessions* in terms of these three purposes, and what it comes up with is both fascinating and convincing. I particularly admire the clarity, ingenuity and surprising precision with which he is able to explain the succeeding episodes and unfolding stages of Rousseau's life. Most worthy of mention is the superb treatment of the imagination; for the two books, Rousseau's and Kelly's, are in a sense nothing but long, complex meditations on the unappreciated danger and promise of this faculty. All told, it is an impressive accomplishment. Kelly has finally succeeded in explaining why it was necessary for Rousseau to write the *Confessions* and for us to read it.

ARTHUR M. MELZER

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The State in Modern Society: New Directions in Political Sociology. By Roger King, with a chapter by Graham Gibbs. Chatham, NJ: Chatham House, 1986. $25.00 cloth, $12.95 paper.

The aim of Roger King's book is

> to suggest that a major dissatisfaction with orthodox political sociology has been its neglect of the state. New approaches in political sociology have recognised the need to return to earlier concerns with the state as found in both Marxism and Weberianism. In contrast to classical interpretations, however, contemporary political sociology is characterised by a more sophisticated account of the relationship between state and economy than found in earlier Marxism, and recognises the state as a structured source of inequality and power, and not simply a reflection. Moreover, there is a less satisfied conception of the nation-state than found in early Weberianism and increased awareness of international and "world" systems. (p. xiii)

Consequently, King believes that there has been something of a "*rapprochement* between Marxism and Weberianism" (p. iii), and that the orthodoxy has withered in the face of a double-barreled challenge from theories of the state.

According to King orthodox political sociology was rooted in postwar behavioralism and urged a revision of classical notions of participatory democracy, which were held to be wildly unrealistic for mass industrialized societies. King reviews this line of reasoning and the response by advocates of participatory democracy, relying heavily on others' characterization of a familiar debate. This may explain the sketchiness of his account. Curiously, he does not comment on Giovanni Sartori's *Democratic Theory,* one of the more influential revisionist statements of the period. Nor does he consider the evolution of the orthodox position, which is evident in the new version of Sartori's book, and especially in Robert Dahl's (1984) *Preface to Economic Democracy,* which stands some distance from his earlier *Preface to Democratic Theory.* As a result King's assessment of the orthodox position and its weaknesses is not entirely persuasive.

King rightly senses that the concept of power is central to his argument. Recent theories of the state reject a behavioral understanding of power as inadequate for analyzing modern societies. Unfortunately, King postpones this discussion until much later in the book, where he reiterates Lukes's position on the "three faces of power," albeit with some reservations and interesting asides on its applicability to community power studies. By delaying this discussion, however, King deprives the critique of orthodox political sociology of much of its force and is led simply to declare the orthodoxy dead so as to get on with the main business of the book, which is to outline certain "new directions in political sociology," all emphasizing the structure of power and the place of the state within it.

At one level this involves an effort to describe the evolution of the modern state, which King does by comparing the historical emergence of the state in different countries. The point of his comparison is to show that capitalism is not always associated with, nor supported by, liberal democratic states. In making this case King draws freely on recent work by Anderson, Badie and Birnbaum, Poggi, and others whose conclusions are presented as more or less definitive accounts of the emergence and development of the modern state. Regrettably, the comparative dimension of King's analysis is quite underdeveloped, as he concentrates on the British case, which, interestingly enough, was one in which capitalism and liberal democracy *were* commingled.

At a second level King's discussion centers on theories of state development. Indeed, he alternates between chapters that are mainly historical and chapters that focus on efforts to comprehend historical change in theoretical terms. The latter sort of chapters are by far the strongest in the book. King's thematic treatments of corporatism, neo-Marxism, and to a lesser extent pluralism are by turns sympathetic and critical, stressing useful insights and problematic contentions. The breadth of his commentary is impressive, and the book will undoubtedly prove very useful for advanced undergraduate and graduate instruction.

King's treatment of the "local state" in Great Britain is especially interesting—not surprising, given his expertise in this area. His explication of the dual state structure in Britain is incisive and balanced, and it shows how corporatist forms of intermediation at the national level may be linked to pluralistic forms at the local level. In turn, this raises questions about the extent to which the local state is simply a branch or extension of the capitalist state. For King such questions highlight the sense in which the local state, while constrained in important ways, is also open-textured. In important ways the local state is an indeterminate one, though that indeterminancy is far from unlimited.

There follows "The State in an International Context," a chapter written by Graham Gibbs. The leap from local to international political economy is not as great as it seems, mainly because Gibbs—like King—eschews theoretical orthodoxy in favor of a contextual analysis of state autonomy. Gibbs rejects approaches that emphasize "internal" explanations of state forms and functions (like modernization theory), but he also criticizes dependency and world systems theorists who simply "read off" the activities of the state from their understanding of an international political economy. Instead he advocates a state-centered approach that is sensitive to the fact that states mediate internal and external forces in unique ways. As Gibbs says, "There is no typical state reaction because in the end there is no typical state" (p. 239), a conclusion tempered by his insistence that "the internationalisation of capital imposes structural imperatives on states which lay down limits" on what state elites may do (p. 238).

On this point, King and Gibbs are in agreement, and it would have been extremely helpful if they had elaborated this theoretical position. Unfortunately, King makes no attempt to map out this *rapprochement* between Marxism and Weberianism in any systematic way. He ends the book with a mercifully brief and decidedly tendentious postscript on the new cold war. If nothing else, this chapter illustrates the tendency among those who study the state to colonize virtually all regions of contemporary political discourse. On the other hand, by refusing to explicate his own theoretical position, King misses an opportunity to make the book more appealing to scholars working in this area.

RUSSELL L. HANSON

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Political Discourse: Explorations in Indian and Western Political Thought. Edited by Bhikhu Parekh and Thomas Pantham. Newbury Park: Sage, 1987. 314p. $27.50.

Discourse analysis has emerged as an approach to the study of meaningful human action that challenges prevailing orientations in fields as diverse as literary criticism, anthropology, sociology, and politics. With intellectual sources as formidable as Saussure, Heidegger, Chomsky, Barthes, Habermas, and Levi-Strauss, among others, discourse analysis has seemed capable of offering a powerful and unifying opposition to approaches extending from behaviorism to the New Criticism. In the social sciences, the routinization of the behavioristic revolution, with its strong tendency to deal convincingly with only a limited range of human activities, has created a vacuum that might be filled by a theoretical apparatus suited to the most obvious and universal of meaningful actions: language.

This collection of essays promises to make its contribution to filling the vacuum in a most interesting and cosmopolitan manner. Abstract conceptual essays are juxtaposed to the analysis of specific cases taken from Indian political and intellectual experience. Unfortunately, the book is so uneven in quality and soft in its focus that its overall impact is to reduce rather than strengthen one's hopes for discourse analysis as a distinct alternative to currently dominant approaches.

The 20 essays in this volume are divided into three parts. The first, "Political Theory after the Linguistic Turn," contains 8 pieces on the relationship between the analysis of political discourse and such well-established traditions as phenomenology (by Hwa-yol Jung), liberalism (by Terchek), and critical theory (by Bhargava). In fact, essays in this section cover a strange and not very coherent array of topics. Minogue, for example, provides an imaginative defense of parliamentary democracy as a "talking shop," and Baxi comments with insight on our tendency to tolerate sycophancy in private relationships but not in political communication. Elsewhere in this section, however, we have essays on Hobbes (Ball), Locke (Terchek), and Marx (Nielsen) that are mainline political theory and neither owe much nor add much to discourse analysis.

The second part, "Critical Discourse and Political Praxis in the West," contains 6 essays of even less coherence. Bay, for example, takes the opportunity to denounce, with no particular originality, U.S. national security policy. Greven does little more than summarize the controversy between Habermas, the utopian seeking a discourse free of domination, and Luhman, the realist reconciled to the inevitability of power. But in the midst of these is Ashcraft's closely argued and extremely useful essay explaining and assessing the developments that led the critique of positivism to focus on the problem of meaning and communicative action and the necessity, as he sees it, to construct an empirically grounded approach to the interpretation of meanings in everyday life.

The last part of the book is called "Emancipatory Discourse and Satyagraha in India." In this section one expects to see the application of abstract theory to the specific case of Gandhi's political activity. While the essays are sometimes of considerable interest, they are mostly analyses of Gandhi's thought or that of his associates, and—apart from the use of the word *discourse*—they owe little to any tradition except general conceptual analysis. Nevertheless, one can learn much from these essays, and Parekh's study of Gandhi's strategy of reform is especially rewarding.

This volume of essays is held together less by a clear and coherent commitment to analysis of discourse as a distinctive intellectual approach than by a shared antipathy to empirical social research—"positivism," as it is invariably labeled. In some cases, for instance, the essay by Nandy, "Cultural Frames for Transformative Politics: A Credo," the antipathy extends to science in general, even natural science. *Discourse* becomes a flag rallying opponents not only of behaviorism but of technology and Western power. Most of the authors are not guilty of this, but the looseness of the concepts of "discourse" and "positivism," as used in this book, invites the juxtaposition of essays of widely different intention and intellectual quality. There is much of value in this unusual collection but less than is needed if the analysts of discourse are to achieve the impact they seek.

FRANK MYERS

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Another Liberalism: Romanticism and the Reconstruction of Liberal Thought. By Nancy L. Rosenblum. Cambridge: Harvard University Press, 1987. 225p. $25.00.

Another Liberalism is a richly textured, elegantly written, complex, and at times provocative book. Assuming familiarity with the liberal tradition and drawing from a broad range of English, French, and German romantic literature, Rosenblum makes two principal arguments. First, the sentiments, values, and mode of life of those she calls "romantic sensibilities" require a liberal political setting. Despite their antagonism toward some of its central tenets, chastened romantics must make their peace with, or "come home to," liberalism. Second, those who critique conventional liberal thought from the romantic perspective considerably enrich it. "Another liberalism" is liberalism "reconstructed" in response to this challenge: "It does not refer to a formal tradition of thought; its coherence is psychological, and not a matter of intellectual history" (p. 3).

Part 1 accentuates the clash between romantic yearnings and liberal values. Romantic militarism, with its nostalgic hope of escape from the ennui of life in civil society through the attainment of heroic glory in battle, conflicts directly with the liberal emphasis on peace and security. The descendant of such heroic longings—military self-assertion—also conflicts with liberal politics: whereas liberalism looks toward the maximizing of the general happiness through the reconciliation of interests, the militant individual's happiness and sense of self depend on distinction from others and conflict with society as a whole. The romantic "law of the heart"—the anarchic desire to obey only the commands of one's feelings and the accompanying hostility to legalism and impartiality—is presented as the most powerful attack on liberalism. Rosenblum's evident sympathy with the romantic sensibility both causes and enables her to extract to the utmost the political relevance of this literature. Yet she is far from uncritical, noting both the pervasive narcissism and the antisocial nature of romantic visions and also the "dark side" of each even when assessed only from the romantic's point of view.

Chapter 3 and part 3 develop Rosenblum's argument that liberalism benefits from the reconstruction it is forced into by its romantic critics. She argues that distinctively romantic notions of individuality—stressing spontaneity, inwardness, and detachment—necessitate rethinking the distinction between public and private. Whereas *private* in conventional liberal thought signifies the social as contrasted with the political, once the influence of romanticism is felt, it comes to denote the realm of the personal and intimate. Part 3 examines three separate facets of the romantic approach to liberalism. The first is heroic individualism as exemplified and celebrated by Thoreau; the second is the notion of "shifting involvements," which values the sphere of public life as a necessary escape from private life, and vice versa; the third is communitarianism, cast in a romantic light and assessed for its contributions to liberal theory.

While presenting all three challenges to conventional liberalism with equal thoroughness and insight, Rosenblum clearly prefers "shifting involvements." In this variant of pluralism, private life as a sphere of intimacy and detachment provides relief from political attachment just as the sphere of political activity and connectedness provides relief from the excessive self-absorption of privacy. As Rosenblum acknowledges, the contributions of heroic individualism and communitarianism are far more problematic. Indeed, the excellent chapter on contemporary communitarianism is so subtly devastating that it leaves little to be salvaged from such theoretical tendencies.

Rosenblum is utterly persuasive in her argument that romantics need liberalism. I find the argument that liberalism has benefited greatly from romantic critiques and insights less persuasive for three reasons. First, it depends on giving "conventional" liberalism less than its due. Rosenblum says, for example, that "classical liberalism . . . does not recognize the emotional intrusiveness of public life" (p. 72). But even Locke, in the *Letter on Toleration*, is concerned about the protection of deeply personal privacies, such as those of friendship, domestic life, and the inward state of the soul. He did not need romanticism to point out the value of these things. Second, it is not always clear that what the romantics discussed really contributed to liberalism. Liberals are usually sympathetic with Thoreau's stand against the state because the cause he stood up for *happened* to be a profoundly liberal one. But what if his romantic anarchist appeal to his conscience had been *against* some liberal cause? On civil disobedience, and in general, the nature of Thoreau's contribution to liberal thought remains murky. Third, it is not apparent that the contributions of some liberal-romantics to liberalism originated from their romantic values. In the cases of Humboldt and Constant, Rosenblum makes a very good case that their most important contributions were inspired by their romanticism. But in the case of Mill I am less convinced. Rosenblum calls concern with self-cultivation "deliberately antiutilitarian" (p. 132). But Mill argues strongly in *Utilitarianism* that the cultivation of the "nobler feelings" is related in two ways to the achievement of the greatest happiness of the greatest number. First, without the development of such sentiments, each person will be incapable of experiencing the highest state of happiness possible. Second, the development of the sentiment of sympathy for others is crucial for motivating persons to act in accordance with the principle of utility. Mill's emphasis on feelings of concern for the happiness of *all* those affected, whether by a person's actions or by a public policy, is far from a romantic preoccupation about sentiment.

This raises a more general question about the relation of romanticism to liberalism. Rosenblum displays abundant awareness of the clash between romantic self-preoccupation and the need of any convincing theory of liberalism to be able to universalize its maxims. She points to the exclusivist, elitist, and necessarily masculinist nature of some romantic visions and to the typical unconcern with justice of others (pp. 21, 40). She notes that the romantic valuing of social diversity can justify cruelty and impoverishment (p. 210, n. 65). Clearly, she is not in agreement with such implications of romanticism, and she knows that we cannot fully assess its actual or potential contributions to liberal theory without confronting these limitations. While romantic needs may be "not felt by everyone" (p. 190), they are needs that *may* be felt by *any*one, regardless of class, race, or sex. Thus *Another Liberalism* raises important new political questions. Before we can conclude that romanticism of any but a self-centered kind can be reconciled with liberalism, the question that is raised at the end of the book needs to be considered further: What distributions of goods and benefits (education and day care, employment opportunities, and decent housing are clear examples) would a society have to ensure for all of its members to enjoy the opportunities for self-development and free self-expression so highly valued by romantics?

Another Liberalism makes us confront fully what the romantic perspective, with all its failings, has to contribute to political thought. In doing so, it made me think harder and question some of my conceptions of liberalism more than anything I have read for a long time. At the same time, the book is a great pleasure to read for both its erudition and its style. I highly recommend it to all students of politics with a serious interest in past or present theoretical debates.

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The Causal Theory of Justice. By Karol Edward Soltan. Berkeley and Los Angeles: University of California Press, 1987. 265p. $30.00.

Don't let the title fool you. *The Causal Theory of Justice* is firmly within the classical tradition of political theory. Following the footsteps of Cicero and Machiavelli, Soltan mingles empirical analysis with normative theorizing. However, unlike theorists of an earlier time, he makes explicit his combination of fact and value, the empirical and the normative. Moreover, in common with many classical authors, he defends natural law as the appropriate foundation for political philosophy, turning his back on the relativity of the postmodern mind.

The book is divided into two parts. Both are of interest to the student of modern political theory and political economy, though the empirical section alone may also prove useful to those interested in wage equity and personnel systems.

Responding directly to Rawls and Habermas, Soltan proposes an appreciation of our natural understandings of justice rather than the construction of veils of ignorance or ideal language acts. To test normative prescriptions, he argues for empirical research into the universal justifications and standards of rightness that individuals find compelling when and if they are free from the forces of self-interest. Norms that are found to be effective under such conditions are deemed "right," as they are capable of receiving widespread consent. The assertion of a natural logic or reasonableness, then, is the anchor of his treatise. Like Habermas, he believes that human beings share principles that are, to be sure, clouded by appetites. Like Rawls, he hopes for simple principles of justice applicable to large and complex organizations, though he embraces natural law rather than a contract as the most reliable basis for such theorizing. That is, he would make Rawls empirical and expand Habermas beyond the language act.

While these suggestions in part 1 respond to current theoretical debates, it is less clear why Soltan proceeds with his second or empirical exercise as he does. The reader anticipates the evolution of a set of normative principles that would then be shown to reside in the mind of the average man when freed from self-interest and other external forces. Rather than this deductive pattern, an inductive approach is taken. There is little logical or rational analysis preceding the empirical exercise. Thereafter, a series of broad-brush empirical presentations concludes that job rankings in the U.S. emphasize their importance and difficulty. We are left uncertain how importance and difficulty are tied to a sense of justice. In fact, the finding that such principles alone summarize common reflection on the issue of the comparative worth of jobs makes one think that the principles we naturally share are few and broad indeed. Yet here again the comparison with Cicero and those who shared his passion for natural law may be relevant. Classical natural law analysis was eclipsed because it could not locate natural principles of sufficient power and discrimination. If all we can know of justice is the basic principles we share when not dominated by self-interest and if those principles are only that one defers to assignments that are important and difficult, this may be too thin a theory of justice to merit ultimate concern.

Indeed, one wonders if the classical line of pursuit, the pursuit of theories of justice or sets of impartial principles, is profitable. Soltan himself suggests rather tellingly that his and similar analyses do indeed reject the possibility that "the highest moral aspiration of humanity may well be a morality of love based on empathy and intuition" (p. 65). Further, he notes that his theory of justice is a second-best morality, possibly most adaptive to powerful, modern, large-scale organizations. As with Rawls and Habermas, Soltan's sense of justice is bent to the service of modern institutions much as Cicero's was bent to the service of one classic institution, the Roman Empire. Could it be that no theory of justice, causal or other, is worthy of the name where it seeks a level of generality appropriate to large bureaucratic structures—that justice is only definable in face-to-face communities? The Greek tradition as opposed to the Roman—Plato as opposed to Cicero—suggests this conclusion. One wonders if the nation-state can be just in any but a thin sense.

This is a provocative book and a worthy contribution to studies that synthesize the best of modern theorizing and relate empirical analysis to such theorizing. It is a metadebate as to whether or not principles of justice applicable to large institutions—and therefore, of necessity, at a high level of generalization—are worth the pursuit. To the extent they are, Soltan adds to the ongoing dialogue. It does seem that his empirical sections suggest more than test a sense of justice. A more consistent approach might have put deduction before induction here. Still, this is a book to be read and thought through as a significant way station in our evaluation of neoclassical thought.

ROBERT BENEDETTI

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The Possibility of Cooperation. By Michael Taylor. Cambridge: Cambridge University Press, 1987. 205p. $39.50 cloth, $12.75 paper.

Michael Taylor published an earlier version of this work, entitled *Anarchy & Cooperation*, in 1976. His basic thesis, lucidly presented in both editions, is that the Hobbesian problem of order can be interpreted as a repeated *N*-person prisoner's dilemma, wherein order can be maintained (if the future matters) by decentralized conditional cooperation. Thus Hbbes, having failed to analyze the full potential of decentralized regimes, embraced the solution of the coercive state prematurely.

Though the fundamental argument is the same, the new version has been reworked in several significant ways. First, Taylor provides a more detailed exposition of the differences between collective action problems and the prisoner's dilemma. This is one of the most valuable parts of the book. Formal modelers can easily become fixated upon a particular game, such as the prisoner's dilemma, and try to analyze every collective action problem in these terms. Chapter 2 analyzes circumstances (payoffs) that constitute a prisoner's dilemma; circumstances that create other games, notably chicken (which, unlike the prisoner's dilemma, does not exhibit a Pareto-inferior equilibrium); and still milder problems such as coordination games (which, unlike chicken, need not involve conflict of interest). Thus readers are sensitized to a wider array of collective action problems.

Second, he has strengthened some of his earlier results on the sustainability of cooperation. In the first edition a typical result was that cooperation by all parties could be enforced only if a player's immediate advantages of defecting today were outweighed by the long-run costs of foregoing future cooperation. Now Taylor has established sufficient—as well as necessary—conditions for maintaining cooperation as an equilibrium. The difference is substantial. For necessity, he had to show only that a vector of conditionally cooperative strategies was secure against a specific deviation, for instance, someone defecting forever. To prove sufficiency, Taylor proves that a cooperative vector is secure against any player deviating to *any* other strategy.

Third, the technical chapters have been rewritten to make them easier for nonmathematical readers. (Such readers may, however, regret the deletion of the old version's chapter 5, which was an informal summary of the mathematical analyses.)

Fourth, there is a bit more emphasis on the difficulty of securing cooperation by a decentralized structure. Taylor's more sober assessment, signaled by his careful choice of a title, partly reflects a comparison with the optimism of Axelrod's *Evolution of Cooperation* (1984). But his concerns predated Axelrod's book; indeed, they are most developed in Taylor's earlier work, *Community, Anarchy, and Liberty* (1982).

In other respects the book is unchanged. Perhaps more importantly, the same strategies (an *N*-person version of tit for tat, permanent defection, permanent cooperation, a class of trigger strategies, and a "nasty" tit for tat that initially defects) are examined. In some ways this is not a serious limitation. With just these, Taylor can explore the symmetric extreme outcomes of universal cooperation and universal defection and specify when they are in equilibrium. He can also explore a few asymmetric possibilities, for instance, where two blocs of players alternately defect and cooperate. But the strategy restriction can lead readers into underestimating the number and variety of equilibrium outcomes. An important result in game theory, the Folk theorem (so named because it is part of the oral tradition of game theory) states that in repeated games of this kind, *any* outcome can be sustained as an equilibrium if the future is sufficiently important, so long as each player gets at least his "security level," that is, the payoff a player can guarantee himself by defecting permanently. This implies that a very asymmetric outcome can be an equilibrium. Taylor recognizes that there are many equilibria and that players consequently face the problem of coordinating on the same equilibrium, but the Folk theorem indicates that the situation is much worse than he implies. (One may want to use Schelling's idea of focal points to rule out some of these equilibria as bizarre theoretical possibilities, but as Taylor notes, this argument takes us well outside the confines of game theory.)

The second point of continuity is that the new edition continues to be an equilibrium analysis. Unlike Axelrod's ecological examination of strategy populations, *The Possibility of Cooperation* says little about dynamics. This would be unimportant if (1) there were only one equilibrium in the repeated game or (2) there were many but they could be Pareto-ranked, so that one could plausibly assume that the players would choose the Pareto-superior equilibrium. However, because neither condition holds here, the issue of dynamics—toward which of many equilibria will a system move?—becomes crucial.

Finally, the formal models continue to assume perfect information: everyone in a group knows how many members cooperated. Yet as in the earlier work, Taylor's informal intuition is that "such monitoring becomes increasingly difficult as the size of the group increases" (p. 105), an intuition that has been validated by recent formal work.

Taylor's appropriately cautious points about the fragility of decentralized methods of securing cooperation leave the reader in some confusion about his conclusions. On the one hand, his careful exposition of the nature of collective action problems, showing that they are not all prisoner's dilemmas, encourages him to argue that he is analyzing the most difficult type of collective action problem. He therefore claims that the analysis is biased in the right direction: even if one assumes the worst, the repeated game analysis shows that decentralized strategies can sustain cooperation under certain conditions. Thus, even granting Hobbes the most pessimistic assumptions about the state of nature, one can show that the Leviathan is not the only solution.

On the other hand, the multiple equilibria and monitoring problems noted above must also be entered in the ledger book, and they appear to count against decentralized solutions. And because Taylor's formal models deemphasize the first problem and omit the second, they are not in fact a worst-case analysis of collective action problems. Hence it remains an open question whether Hobbes' opposition to a purely decentralized regime (anarchy) was unsound.

Of course this does not rescue Hobbes' coercive alternative. Indeed, had Taylor turned his game-theoretic tools on the centralized solution, he might have revealed that in addition to well-known empirical and normative problems there are interesting strategic weaknesses in Hobbes' argument. For example, how would players in the state of nature agree on a *particular* sovereign? (John Ferejohn has argued that there is an underlying coordination-on-an-equilibrium problem here, given that different rulers could implement different outcomes.) And could the players credibly commit to obeying the ruler? (This issue has been recently explored by Jean Hampton in *Hobbes and the Social Contract Tradition* [1986].)

But it is unfair to ask an author to write another book. By showing how a game-theoretic analysis of political institutions both sharpens our understanding of classical arguments and advances new ones, Taylor has already made a significant contribution.

JONATHAN BENDOR

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Political Ethics in Public Office. By Dennis F. Thompson. Cambridge: Harvard University Press, 1987. 263p. $25.00.

Political Ethics in Public Office collects several essays previously published by Thompson that have been reworked to yield a volume with considerable continuity of theme and subject matter. Thus it can be read from front to back with greatest profit, but individual chapters stand on their own and can be studied out of order without real loss. This versatility makes the book doubly valuable since many of the chapters in it will be starting points for anyone who wants to reflect seriously on the duties of legislators, the responsibilities of bureaucrats, and standards of behavior for public officials.

The broad topic of the book is the ethical requirements and tensions of public office, but the common theme that threads through most of the essays is responsibility: What is it? Who has it? How do we locate it? and How is it affected by political democracy? The first chapter argues that an officeholder in a democracy who acts as authorized by the office or position has no greater responsibility than do citizens for transgression of moral principles (p. 22). Chapter 2 directly addresses the problem of assigning responsibility to individuals in bureaucracies where decision making and policy execution are diffused through many offices. Chapter 3 asks whether we can hold officeholders criminally as well as morally responsible for the consequences of their behavior. Chapter 4 surveys answers to the question of whom the legislator is supposed to represent. Chapter 5 looks at how much privacy we can expect public officials to surrender. Chapter 6 rehearses the difficulties in justifying paternalistic regulation of individual conduct. Chapter 7 points out the responsibility of government officials to ask certain questions and allow for certain unexpected contingencies before involving people in income maintenance studies and other similar social experiments.

The discussions throughout are careful and measured, conversant with a wide literature, and full of useful distinctions that allow many stalemates and logjams in the public understanding of political ethics to be bypassed or broken through. Even if readers are unconvinced by Thompson's particular views, they will find in the essays indispensable tools for mounting alternative conclusions.

If responsibility is Thompson's motif, democracy is his leitmotif. In a democracy citizens are unusually implicated in the bad behavior of their leaders because they directly or indirectly authorize or consent to much of it. Moreover, the standards of conduct we apply to the behavior of officials are driven, in part, by democratic accountability. This is especially so regarding assignments of responsibility. Thompson prefers one account of responsibility ("personal responsibility") over two other accounts ("hierarchical responsibility" and "collective responsibility") because it "offers a stronger foundation for democratic accountability" (p. 64).

"Personal responsibility" conforms to the presuppositions of traditional moral responsibility: individuals must have both caused something to happen and been able to do otherwise (pp. 47, 48). One of Thompson's criticisms of "hierarchical responsibility"—which makes the highest official in the chain of command responsible—is that it does not "coincide with moral responsibility" (p. 43). Thus, "hierarchical responsibility imparts scarcely any moral force," encouraging the "ritual" of leaders quite facilely declaring themselves "fully responsible" for mistakes under their administration (p. 43). However, the emptiness of this ritual, I suggest, stems less from the fact that such facile admissions do not track with moral responsibility than from the fact that no material costs attach to them. The president, for example, does not lose office for the bunglings and crimes of his underlings, so why not "take full responsibility"? The real solution is putting some punch into such "taking." If we think about officeholders with democratic accountability in mind, why should we be more concerned to see that their liability to penalty coincides with their moral responsibility than that it most effectively spurs them to minimize bungling and crime under their administrations? Thompson acknowledges that there may be reasons for severing liability from responsibility (p. 43) but assumes that democratic accountability does not especially fuel such a separation. The liability of officeholders to merited praise and blame may do less for democratic accountability than would their liability to get the boot whether they are personally blameworthy or not.

That the reader may be led to try out such speculations is part of the pleasure of reading *Political Ethics in Public Office.* Its arguments allow dissenting explorations. Yet its own views have such resilience that all dissenters and speculators may find they have returned to the fold in the end.

ROBERT K. FULLINWIDER

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The Grammar of Justice. By Elizabeth H. Wolgast. Ithaca: Cornell University Press, 1987. 219p. $29.95 cloth, $9.95 paper.

With this book Elizabeth Wolgast joins the contemporary debate about the limits of liberal theory and the implications that those limits have for our ways of thinking about ethics and politics. Taking exception to a Rawlsian general theory of justice, Wolgast joins Peter Winch, Alasdair MacIntyre, and Ludwig Wittgenstein in emphasizing the contextual, cultural background conditions of justice. While liberal theory holds out the theoretical vision of justice as a universally applicable norm, Wolgast contends that in reality our moral experience informs and limits what we can formulate abstractly. Practical dilemmas and problematic facts give rise to our philosophizing about justice. And our actual moral reasoning about justice in such situations is heavily influenced by our culture, language, history, and personal background. Following Wittgenstein, Wolgast refers to these factors as the "grammar" of justice.

Addressing U.S. political tradition and culture, Wolgast presents us with a series of tightly argued philosophical investigations of terms central to U.S. thinking: *social contract, respect, individual rights, freedom, majority rule, political representation:* "The goal of these essays is to get a clearer view of how our moral reasoning works and how it in turn illuminates political judgments" (p. xiii). She particularly wants to show how basic liberal assumptions shape and limit our political and legal thought.

According to Wolgast, the U.S. colonists inherited from seventeenth-century English and Continental philosophers a model she calls "social atomism," a fundamental conception of society as a simple collection of independent, self-motivated units. Wolgast argues that atomism may not be the only or best way to understand human society. It assumes the primacy of self-interest and the natural unconnectedness of people. On the atomistic view, community is said to be merely the aggregate of self-contained, self-interested individuals who are equal in the sense that they are interchangeable. As Wolgast notes, the application of this concept of equality to human beings, given sex differences, is problematic. The burden of her argument here is that it is unjust to treat dissimilar cases in similar fashion.

So far, there is nothing particularly new or startling in Wolgast's discussion of social atomism. She simply reiterates the by-now customary critique of the methodological individualism that is a presupposition of liberal thought. However, she now begins to expand the critique of liberalism by examining controversial issues of contemporary society.

For example, in chapter 2, entitled "Wrong Rights," Wolgast points to one consequence of the atomistic model: the singular emphasis we put on individual rights as a way of addressing injustices. In liberal thought rights are viewed as possessions, and the individual person, as bearer of these rights, must claim them as his or her due. This is all well and good in situations of equality among peers, she contends. But rights-language is limited in situations of inequality, for example, in patient-doctor or parent-child relations. In these situations society should not expect the weaker party to assert its rights but should place the burden of discharging duties upon the more powerful person. However, since, under the model of social atomism, individuals can be counted on to act only or primarily in terms of self-interest, the notion of duty and responsibility may not sit well with the doctors and parents. For this reason theories of individual rights derived from an atomistic conception of society do not go very far toward realizing justice in society.

In subsequent chapters, Wolgast examines, from the same critical perspective, the concepts of democracy, majority rule, representative government, and punishment. In chapter 5, for example, she presents a novel argument that incorporates what has come to be called a feminist critique of pornography. This argument is significant and merits detailed analysis.

Wolgast, a liberal supporter of First Amendment freedoms of speech and press, nevertheless understands sympathetically the radical feminist objection to pornography that portrays bondage, torture, and murder of women for sexual stimulation and pleasure. What is objectionable is not just these representations of brutal sexual treatment of women, she argues, but the lack of a context in which they are understood to be reprehensible and condemnable. "Without some such context, the representations carry the message that such treatment of women is all right" (p. 107). Wolgast contends that this kind of objection carries enough legal weight to justify control of the objectionable materials.

Wolgast maintains that the justification for censoring pornography is not rebutted by an appeal to the First Amendment. Central to her argument is her contention that the appeal to the sensibilities of "the average man" to judge whether pornography is offensive is exactly that: an appeal to the average *man,* not the average woman. Wolgast acknowledges that pornographic materials may be seen differently by one group than by another. They may be felt as insulting by one group but inoffensive by another. It is crucial for her argument that such differences in perception be acknowledged as a social reality and that our understanding of what it is to treat everyone with respect should allow for such differences in the perception of respect.

Seeking to translate her moral argument against pornography into constitutional terms, Wolgast notes that the connection between respect and constitutional action has already been made in *Brown* v. *Board of Education* (1954). Because racial segregation was perceived to be demeaning and disrespectful, the Court rejected the justice of "separate but equal" facilities. The insult perceived by blacks took priority over protests of innocence by those charged with offending. Similarly, the insult perceived by women has priority over the protests of pornographers who claim no intent to demean or offend women. Insofar as pornography is felt by women to be demeaning and insulting, its protection by the government under the First Amendment cannot be easily argued in a liberal society committed to equal respect for individual persons. In this manner Wolgast uses the premises of liberalism to argue for appropriate censorship of pornography and against the libertarian case for laissez-faire. This is an imaginative, constructive argument made all the more provocative and paradoxical by Wolgast's appeal for support to Mill's *On Liberty* and *On the Subjection of Women*—essays usually cited by libertarians to support noncontrol of pornography.

Wolgast's main contribution is her use of Wittgenstein's philosophy to illuminate our understanding of morality and justice. Although liberalism has many positive aspects, the liberal ideal of universally applicable moral norms is plainly unrealistic and untrue to actual life because it ignores language, history, and culture. Her critique has implications for ethics, political theory, and moral education.

Wolgast's analysis is marred occasionally by a lack of historical perspective. She ignores the republican tradition of civic humanism in U.S. politics. She also fails to take into account the influence of capitalist ideology on U.S. political and social thought. At the same time, her Wittgensteinian critique of the abstract conception of justice present in much contemporary liberal theory is convincingly made. And her discussion of pornography and censorship is a valuable contribution to contemporary debate. Students of contemporary political thought, social philosophy, and civil liberties will find her work stimulating and thought-provoking.

MARY C. SEGERS

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AMERICAN POLITICS

And We Are Not Saved: The Elusive Quest for Racial Justice. By Derrick Bell. New York: Basic Books, 1987. 288p. $19.95.

"The harvest is past, the summer is ended, and we are not saved." Having "taken his text," as in the tradition of the black preacher, from the lament found in Jeremiah 8.20, Derrick Bell has written a most absorbing and compelling book. The book is many things but is foremost a profound commentary on the question of race in the United States, expressed in an examination of the various political and legal strategies adopted in pursuit of achieving full rights and protections for black citizens. The book takes the unusual form of a series of smaller allegories within a larger allegory. It is thus a highly imaginative and largely successful use of fictional characters and circumstances to illumine by means of critical analysis problematical issues in the implementation of racial public policies and to dramatize by means of intriguing events the persistent barriers and impediments to realization of the goals of these policies.

Bell's chronicles, as the smaller allegories are called, provide insightful assessments of a range of strategic considerations proffered or engaged in by blacks as a way out of their subordinate status in the United States. The analytical fecundity of the chronicles is made more engaging by their titles, which include such suggestive allusions as "The Sacrificed Black School Children," "The Ultimate Voting Rights Act," and "The Twenty-Seventh Year Syndrome." The chronicles are the feedstock for an extended discourse between two learned and experienced lawyers that yields thoroughly insightful and compelling analyses of the development of legal precedents in structuring and sometimes subverting the scope and meaning of racial public policies.

To the adherents of contending strategies for black liberation, the author offers a sobering, consensual conclusion: "While the central motivating theme of black struggle is faith, the common thread in all civil rights strategies is eventual failure" (p. 248). With that truism as a prelude, the ending of Bell's book might be characterized by some as a cop-out. The ending quite arguably lacks the creativity of insight and incisive analysis that is shown in much of the rest of the book. For many, the perspective and the language of the final segment will lack the cogency they expected, and the absence of an analytically compelling wrap-up will no doubt be disappointing given the highly suggestive title of "The Ultimate Civil Rights Strategy."

Clearly, a word is due about the method employed in this book and its appropriateness for scholarly presentation. The extended use of fictional scenarios as the exclusive method for the presentation of scholarly analysis is not without precedent but is nonetheless not a commonplace approach. To be certain, Bell's scholarship is no less sound for use of the allegories. The characters' dialog contains specific and extensive references to actual source materials. Thus the work is fully documented in accord with the most exacting standards of scholarship. Indeed, the use of the allegories make intriguing and understandable what might otherwise have been a tedious, legalistic treatise of interest to a very limited audience of scholars. Perhaps the major disadvantage of the method is that scholars and others will not quote the author per se so much as they will likely remember the allegories and their morals. The author's personal perspectives on specific issues are camouflaged by the characters, who give contrasting and opposing viewpoints. The advantage of this is that the arguments presented in the chronicles become less polemical and more didactic. The substance of the book leaves no doubt about who Derrick Bell is—preeminent legal scholar and social critic, frustrated playwright, and ever hopeful christian mystic. This book will be widely read and long remembered.

Bell's book is clearly directed toward a broad audience of scholars and concerned laymen. It is a mix of scholarly and literary styles. It is neither wholly political science, legal exegesis, social criticism, or historical analysis but a blend of all these. Thus there is no single explicit thesis so common in disciplinary-specific works. The book obviously provides grist for constitutional law classes but moreover provides an excellent context for the discussion of many themes of particular significance to the field of black politics. In this regard it provides a kind of updated perspective on concerns raised in the twin volumes by Matthew Holden, *The White Man's Burden* and *The Politics of The Black Nation.* Bell's book makes a major contribution to the cross-disciplinary literature of changing race relations and joins the ranks of such stellar works as Harold Cruse's *The Crisis of the Negro Intellectual,* Joel Williamson's *The Crucible of Race,* and Cruse's latest work, *Plural but Equal.*

GEORGIA A. PERSONS

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Policy Analysis by Design. By Davis B. Bobrow and John S. Dryzek. Pittsburgh, PA: University of Pittsburgh Press, 1987. 246p. $27.95 cloth, $13.95 paper.

Policy design refers to a process through which ideas are converted into fully developed policy alternatives or to the products ("designs") that emerge, such as laws, regulations, and programs. Policy design has been a much-neglected topic of study, and Bobrow and Dryzek's book contains the most comprehensive discussion produced to date.

In the first part the authors examine how policy design proceeds under five different frames of reference: welfare economics, public choice, social structure, information processing, and political philosophy. The authors' presentation is enriched with the use of an example in which policy analysts, operating from each frame of reference, design alternatives to resolve the air pollution problems in a hypothetical city, Smoketown.

Bobrow and Dryzek's interpretation of welfare economics and public choice are relatively standard, albeit interesting, treatments. Each, however, is judged insufficient as a comprehensive guide to policy because of unwarranted and simplistic assumptions that are seldom met in practice and (for public choice) a too-rigid view of the policy process. In the social-structural approach, policy alternatives are not judged by their efficiency or responsiveness, as in welfare economics or public choice, but by their impact on the relative standings of individuals and groups in the community. Hence, this approach admits many values and is better able to illuminate policy consequences in terms of who gets what —including money, power, status, expertise, environmental quality, and other values that are being distributed. In the political philosophy frame of reference, policies are judged by ethical premises that emphasize the intrinsic rightness or wrongness of policy alternatives. Bobrow and Dryzek contend that neither the sociological nor the philosophical frames of reference are sufficient guides to design—for a variety of reasons but especially because they are better suited to raising issues than to resolving them.

Information processing approaches differ from welfare economics and public choice in their rejection of the assumptions that individuals are self-interested utility maximizers. Instead, these approaches assume that individuals are far more complex and rely on decision strategies that often introduce bias into their decision making. The authors present several strands of the information processing approach, ranging from an optimistic version characterized by the belief that policy analysis can improve the rationality in decision making and thereby improve policy choices to pessimistic views that only incremental policy changes are possible or desirable. Neither of these is sufficient for all policy situations, according to the authors. The more optimistic approaches may not be acceptable or realistic in some political contexts, but the pessimistic views, they contend, "are destined to leave the world pretty much as they found it" (p. 95).

In the second part of the book the authors turn their attention to epistemology and discuss positivism, piecemeal social engineering, eclectic policy analysis, forensic policy analysis, relativism, accommodation, and critical theory. As in the first section, the authors find fault with all approaches, although acknowledging that each has some redeeming values.

The authors' approach, presented in the final portion of the book, rests on the premise that an appropriate frame of reference and an appropriate methodology will vary depending upon the values and the context. Their suggestions about values are quite appropriate. One must consider timing (when should the end be sought), amount (how much is "good enough"), and relative priority of competing values. Context needs to be captured, according to Bobrow and Dryzek, in terms of its level of complexity and uncertainty, the feedback potential, degree of control available to individuals in the policy process, stability of actors and interests, and the characteristics of the audience, which includes those who adopt, implement, and react to the policy.

Some values lend themselves to a particular frame of reference and to a particular methodology. If the primary value sought is economic efficiency, welfare economics may offer a useful set of deductive principles to guide policy design, and causal analysis may be appropriate for determining effects. But where there are multiple values, complex situations, ambiguous results, and controversy, then other approaches must be used. When goals are unstable and conflicting, the authors suggest that incremental change would be more appropriate, combined with forensic or critical policy analysis.

This is a valuable book on policy design. It brings into focus in a clear and compelling manner the implications of alternative frames of reference and methodologies. The authors argue that there are no simple prescriptions for policy other than that the approach selected must have a good "fit" with prevailing values and with the context within which the policy must operate. They list the conditions needed for each of the frames of reference and epistemologies but acknowledge that these seldom hold in practice. They do not, however, stand the process on its head and identify the most appropriate approaches to design, given a particular constellation of values and contextual issues. Perhaps that should become part of the research agenda for policy design.

ANNE SCHNEIDER

Oklahoma State University

Policy Evaluation for Local Government. Edited by Terry Busson and Philip Coulter. Westport, CT: Greenwood, 1987. 266p. $39.95.

The purpose of this book, say editors Terry Busson (director of the Institute of Government at Eastern Kentucky University) and Phil Coulter (director of the Institute for Social Science Research at the University of Alabama) is to present some of the best innovative research in local government policy evaluation. They subscribe to the notion that such evaluation can contribute to policy success, especially in the face of current declining public resources, by helping managers decide what services should be reduced or cut out, which staff should be reassigned, and where changes in operations should occur.

Lest you think that the editors are not aware of the literature that concludes that most policy evaluation does not have much immediate impact on these kinds of decisions, I should hasten to add that they discuss the perils and pitfalls of evaluation. They discuss such examples as policymakers' using evaluations as a screen for their political decisions and the resistance of administrators to being evaluated. Still, they are optimistic about the use of policy evaluation as a practical tool for management.

How has evaluation been used? Morgan and England describe how citizens were used in Norman, Oklahoma to evaluate the effectiveness of the city's community development block grants program. They find that citizens gave it lower ratings than city staff did. They also find that citizens are not very knowledgeable about these programs. Stipak discusses the use of subjective evaluation measures (such as citizen opinions) and says they should be balanced with the use of objective measures. Gardner and Feorestana describe social evaluation techniques but agree that they should be balanced by the use of technical (objective) measures. Elaine Sharp finds that a small-sized city did not use subjective or objective measures in deciding how to allocate grants to nonprofit organizations that deliver city services. They based their decision on United Fund allocation patterns. Lorenz and Rogers find that the federal government had little effect on building local government capacity. Coulter looks at the distributional effects of local government decision rules. Toulmin finds that Savannah, Georgia was able to target its urban public services more equitably. Rubin, Rubin, Dobson, and Grush look specifically at the factors promoting utilization of evaluations and come up with a list of nine points that should be followed if evaluations are to be utilized. Downes finds that a local government in Oregon used politics rather than evaluation in their reaction to fiscal stress. Thompson measured the outcome of an economic development project in Battle Creek, Michigan and found it did not create new jobs. Finally, Brudney finds that coproduction is not a simple solution to fiscal stress.

Most of the chapters are well done and contain a lot of research and data. But not all of the articles address the main theme of how program evaluation can or cannot help local governments make decisions in the face of fiscal stress. For example, the Brudney article, while a fine piece of research in its own right, is not really related to the main theme, nor are the Thompson and Downes articles. Nevertheless, several of the chapters would be useful in courses in policy analysis and evaluation or in urban politics. It is true, as the editors note, that government agencies are turning to evaluation for a number of reasons. Thus the book is timely. It contains a lot of original research pieces and these should be of interest to scholars and practitioners alike.

DENNIS J. PALUMBO

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The Personal Vote: Constituency Service and Electoral Independence. By Bruce Cain, John Ferejohn, and Morris Fiorina. Cambridge: Harvard University Press, 1987. 268p. $25.00.

This very good book is certainly essential reading for anyone in the fields of legislative studies, electoral behavior, comparative legislative studies, and British politics. Its focus is on constituency service in the United States and Great Britain but it addresses a remarkably broad range of concerns. Its strength is that it is very rigorously constructed and very vigorously argued. Thus from the focal point of constituency service, Cain, Ferejohn, and Fiorina look out to the evolving institutional structures of both political systems and help us to understand how they work.

The authors argue that constituency service, the representative's ombudsmanlike efforts to help constituents both individually and collectively, is an important form of representation with significant political consequences. Indeed, this sort of advocacy is the most ancient form of representation and certainly deserves a prominent place alongside policy representation and symbolic representation. However, this form of representation may have negative consequences for the process of policymaking: a drift from party responsiveness toward bargaining among parochial interests, a loss of coherency in legislative programs, and a decentralized pluralism among interest groups, which can produce stalemates. Entropy drives representatives away from national institutions and policy concerns and binds them to local and particularistic interests. At the heart of this entropic process lies the electoral incentive.

Based on a series of carefully designed interviews with representatives (administrative assistants of representatives in the United States and MPs and party agents in the United Kingdom) and with their constituents, the book begins by examining the substance of constituency service. The authors find that constituents expect representatives to help their districts and their people. They find that representatives do this work primarily through large staffs whereas MPs do it primarily by themselves. In consequence, more casework gets done in the United States, albeit in Britain MPs spend more time on such activities. They find that representatives in both countries are aware that constituency service can win votes and that the level of effort that representatives devote to casework is related to their electoral vulnerability, to their margins of victory in the previous election. They also find that as partisan ties have declined over the past several decades, constituents have come increasingly to care about constituency service when evaluating their representatives. Correspondingly, representatives have increased their constituency efforts.

The second of the book's three parts investigates the theoretical guts of the matter, the connections between the constituency service activities of representatives and the evaluations and voting decisions of their constituents. Here Cain, Ferejohn, and Fiorina begin by arguing, in general, that "when research results conflict with theory, common sense, or even political folklore, scholars should entertain the possibility that something could be wrong with the research" (p. 123) and arguing, in particular, that where previous research has found little or no evidence that constituency service has electoral consequences, these negative results have been due to unsatisfactory concepts, measures, and methods. They show that voters notice the service activities of their representatives in Britain and the United States and that vigorous performance of these activities enhances the representatives' local reputations. Finally, the authors further show that such activities are not only noticed and appreciated but also contribute to a very significant personal vote in the United States and to a noticeable personal vote in Britain.

The final part of this excellent book is the most creative and stimulating, for it attempts, on the foundations of the analyses that have gone before, to explore the larger implications, including linkages between institutional rules and individual behavior and the consequences for democratic political systems. The authors find democracy in the trend toward increasingly faithful representation of particularistic constituency interests. Yet they conclude that these trends can, unless constrained, produce crises of immobilism and "corrode the conduct of democratic government by undermining the ability of that government to act in ways that improve the lot of its citizens" (p. 229). It is the rules of the game of single-member-district electoral systems, such as those of the United States and Great Britain, that produce electoral incentives for developing a personal vote —and produce thereby inherent tendencies toward decentralization, toward neglecting the interests of the whole for the interests of the parts.

Although this book is unquestionably a genuine and very important cross-national study, it has more to teach us about U.S. politics than about British politics. There is, for example, a usually misleading tendency to assume that the British system is becoming like the U.S.—policy-based voting for MPs, increasingly important incumbency effects, MPs cross-voting in order to pursue personal votes by voting their constituents' policy beliefs. Cain, Ferejohn, and Fiorina treat these ideas as speculative projections rather than as realities. But the fact that they get the realities right does not eliminate the bias: a tendency to interpret other systems as similar to our own or as becoming similar to our own. Another bias that I see in the book is a tendency to overlook the complexity and variability of the motives of political actors; to take seriously only those motives that are individualistic, selfish, and antisocial; and to assume that such preferences are more or less constant across different countries and across different time periods. I must hasten to emphasize, however, that these criticisms, which may sound very critical, are, in fact, very trivial in their consequences for the book's analyses and conclusions. The authors are creative and careful scholars who do not make careless errors. Their book is an absolutely first-rate study. It deserves to be read. And it deserves to be taken seriously.

DONALD D. SEARING

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Biocracy: Public Policy and the Life Sciences. By Lynton Keith Caldwell. Boulder: Westview, 1987. 229p. $33.50.

This highly readable collection of essays by one of the leading scholars in the field of environmental politics and policy provides a surprisingly coherent discussion of the dynamic relationships among scientific discoveries in the life sciences, public policy, public opinion, political theory, and ethics. I say *surprisingly coherent* because the essays were written over a twenty-five-year period. All, however, are "new" in that even those previously published or delivered as professional papers have been revised for this volume. The coherence is also derived from the single-mindedness of the author to make the case for developing a theory of politics grounded in ecological and biological fact and theory. This volume is a welcome addition to the steadily growing body of literature in "politics and the life sciences" and adds significantly to published or forthcoming book-length works by Peter Corning, Roger Masters, Glendon Schubert, Thomas Wiegele and others.

Caldwell's primary purpose is to alert his readership to the vital importance of recent developments in the life sciences and their implications for society. The challenge he describes is one of reconciling the potential discontinuities between biology and culture, as manifested in the new knowledge being presented by life scientists and the existing practices of politics, public policy, and popular culture. In the preface Caldwell states that each essay "is in essence a different version of a common message: Advances in the life sciences are bringing about social changes and conflicts that, in the interest of human welfare now and in the future, should receive more serious and widespread attention than is presently evident" (p. x). This is a strong message, one that is being broadcast with ever greater frequency in the popular media as events and predictions including the nuclear accident at Chernobyl, the spread of AIDS, "nuclear winter," and the "greenhouse effect" increasingly direct our attention to the sometimes precarious but always vital relationship between humankind and the physical environment. Caldwell's concern is nothing less than the "politics of survival." He asserts that the various social responses to biological facts will determine whether we survive in an ecological and biological sense and what form that survival will take. As he points out, "A new level in an ascending hierarchy of knowledge has become necessary, at which the integration of the discrete sciences of man and of life can be accomplished" (pp. 39–40). Caldwell is neither a naive optimist nor an extreme pessimist. Indeed, all his essays reflect a political realism that leads him to assume a cautiously positive posture based largely on a confidence in the ability of humans to recognize the compelling challenges that newly acquired knowledge of nature presents and make principled choices that will promote survival rather than extinction. But hard, informed choices must be made: "If humans fail to find answers to the problem of their behavioral-ecological relationships and to be guided by this knowledge, their future may be too brief to find answers to most of the other perplexing questions about human behavior" (p. 43).

The concept of "biocracy," the origin of which he attributes to a 1933 paper by the biologist Walter Cannon, is the central concept in the book. *Biocracy* is actually defined in several ways by Caldwell. First, in chapter 2 he states that the term "was invented to signify a sociopolitical system in which biological knowledge and technology strongly influence social behavior and thus ultimately affect policy" (p. 19). Later he suggests that Cannon's early formulations constituted an "incipient theory of natural government" (p. 57). Clearly, Caldwell feels that a "naturalistic" theory of politics is what is required. Politics, then, must be made consistent with biological fact and must be seen as an extension of biological theory. This is a challenging proposition for students of politics and is at the heart of those advocating a political science that is a "life science."

Chapter 3, "Health and Homeostasis As Societal Concepts" is a minor tour de force in which Caldwell succinctly reviews a considerable body of literature to make the case for the rigorous application of these concepts to societies and collectivities of societies. Chapters 5, "Life Sciences As Problems for Politics and Law" and 6, "Biocratic Interpretations of Ethical Behavior" provide an excellent introduction to the complex and important issues resulting from new life science knowledge, which dramatically confront our established systems of values (religions, legal systems, governments). A great strength of the volume is Caldwell's insistence on linking political theory and public policy to an ethical foundation, specifically a bioethical foundation. He does not really provide that foundation; rather he makes absolutely clear the need for such a foundation and goes on to sketch a bioethics, the ultimate goal of which is species survival. In this discussion in chapter 6 he addresses three aspects of survival: fitness, homeostasis, and criteria of "right and wrong" in survival (p. 122–33).

For all of its many strengths, the volume does have a few weaknesses. In particular, there is still some confusion in the use of several concepts (biocracy, bioethics, biopolitics, biopolicy, biotechnology, fitness, health, homeostasis, survival), with those concepts never being linked together or cast as a framework of inquiry or explanation. That is to say, there is no formulation of theory provided—only a series of related concepts. Much more work needs to be done on the subject of bioethics and Caldwell has barely begun to incorporate into his formulation the considerable philosophical literature in bio- and biomedical ethics. For example, survival is posited on a collective or group (species) level without confronting the problem of reconciling or making compatible the behavior of individuals and groups or coming to grips with the fundamental tension between competitive and cooperative behavior.

Despite his own awareness of the limitations of previous efforts that have been built upon a synthesis of knowledge gleaned from the life sciences to influence policy (e.g., the efforts of the Club of Rome or the authors of the *Blueprint for Survival* [1985]), Caldwell fails to offer realistic, incremental, concrete, tactical and strategic political recommendations that could move us closer to meaningful public policy. He speaks well to an academic audience, but not to a readership of politicians (though one could hope!). It remains to be seen how advocates and political activists might go about achieving the political goals manifest in a politics of ecology.

SAMUEL M. HINES, JR.

College of Charleston

Governing Prisons: A Comparative Study of Correctional Management. By John J. DiIulio, Jr. New York: Free Press, 1987. 349p. $24.95.

John DiIulio's *Governing Prisons* is superb. It addresses an important question, Under what circumstances are good prisons possible? It examines prison systems in three states; California, Michigan, and Texas. It is grounded in observations at several prisons in each state and illuminated by interviews with scores of prison staff ("keepers"). The interviews are eloquent, the observations keen, the comparisons enlightening, the author's judgments balanced, and the range of scholarship expansive.

The book arrives at a precise, powerful, and important conclusion, "Prison management matters" (p. 256). It also explains why some incidental matters matter less; that is, DiIulio teaches why he downgraded the significance of factors that "sociologists have argued" as causing prisons to be horrible—violent characteristics of the inmates, low level of expenditures, crowding, high inmate-staff ratios, little formal staff training, architecture, the inmate social system, race prejudice, and repressive custodial techniques. These factors did not explain the insecurity and inhumanity afflicting prison life. Rather, they were the conditions out of which good prison managers made good prisons. The secret of good prisons is order, and the secret of order is tireless, careful, rule-abiding, formalized, purposeful, and stout-hearted leadership.

In DiIulio's book the Texas prison system emerges the hero, in the superior and tragic senses of the word. *Superior* because for much of the past two decades Texas's was the safest, most humane, and most productive of the three systems. Texas was "where prison managers effect a strong administrative regime that enforces adherence to the norms and values of a civilized, noncriminal, 'straight' way of life—dressing neatly, washing regularly, being punctual, working hard, speaking respectfully to peers and authorities, delaying gratification for the sake of future rewards" and where "serious disorders are less frequent, meaningful treatment programs more plentiful, and recidivism rates less startling" (p. 7). *Tragic* because the Texas system collapsed into disorder, demoralization, and controversy in the 1980s when less meticulous, forceful, and single-minded administrators were appointed.

The book is outstanding for five reasons. First, it demonstrates that leadership is important and teaches what real leadership is. Dr. George Beto (the Texas director of corrections), E. J. Estelle (Beto's successor), and Wayne Estelle (warden of the best California prison and, incidentally, E. J. Estelle's brother) were meticulous in detail, had vision, set goals, and inspired their staffs to carry out the difficult work of keeping the inmates safe from individual and group misconduct.

Second, it establishes that the philosophies men carry about in their minds are important. Drawing on Aristotle, Burke, and John Stuart Mill, DiIulio shows that the keepers based their practices on distinct and consistent sets of general ideas, which in turn nurtured and honored their adherence to standards of practice. In short, it was ideas about prison conditions, not the material conditions themselves, that affected how staffs behaved. Ideas were the stuff of managing prisons, and what proved truly decisive in making a prison good or bad was the set of answers the corrections staff formulated to profound questions; "Should the inmate be viewed more as an incarcerated citizen or as a confined criminal? Is the rule-abiding prisoner entitled to all, some, or none of the privileges of free, law-abiding citizens? Should privileges be given to him gradually as a reward for good behavior, or all at once upon entering the prison? Do inmates have any right to rebel? Do keepers have a duty to sculpt the moral character of the inmates, or is it enough to prevent overt violence and to encourage rule-abiding behavior? Is criminality caused by such things as personality development and poor economic conditions? Or are most criminals rational actors who believe that crime pays? Or is there a spiritual dimension to criminality such that some criminals can be understood as wicked souls who delight in praying upon the life and property of others? Texas, Michigan, and California keepers," DiIulio continues, "each held a set of ideas on these topics that bound them together and distinguished them from their peers in the other two states" (p. 174).

Third, the book has a comparative and historical perspective. The comparisons revealed that some prisons were good, most horrible. Some succeeded in being orderly and productive, while others failed by being anarchic and brutish. The best were administered with discipline, the worst with such indifference that the convicts took over. DiIulio's powers of description make the reader feel the difference between the security of Huntsville in Texas and the chaos of Huron Valley in Michigan, between the hopefulness of California Men's Colony in San Luis Obisbo and the despair of Soledad a mere 180 miles up the California highway.

Fourth, it celebrates knowledge, not the superficial notions of remote theoretics, but the profound understandings of the keepers derived from their practice. DiIulio teaches the reader how prison life differs from normal society in "the level of human trust and the likelihood of spontaneous, uninduced cooperation among people" (p. 239), and he itemizes the havoc wrought by a federal district judge who, in attempting to cure real ills, disregarded those differences and disparaged the practice of the best of the keepers who heeded them.

Finally, the book is important because it rejects fatalism and determinism in human affairs. It records what real men really did to shape events. "I have suggested," writes DiIulio, "that good prisons are possible and denied that bad prisons are inevitable. I have attempted to identify the administrative conditions under which better prisons are possible and have raised serious questions about the importance of money, training, and numerous other factors that are claimed to have a major bearing on the quality of prison life. I have doubted most of what sociologists have argued about prisons and made recommendations that run counter to most contemporary ideas about how best to handle incarcerated felons. Furthermore, I have claimed that prison order is both good in itself and a necessary precondition for [humane treatment of prisoners]" (p. 255).

At the end of the book DiIulio points to Alexis de Tocqueville: "If [his and Beaumont's *On the Penitentiary System in the United States]* had been studied and its general lessons heeded, most, maybe all, of the last century and a half of America's penological follies might have been avoided" (p. 320). Tocqueville's spirit animates every sentence of *Governing Prisons.* That spirit is the reason why I believe DiIulio's book should be assigned in every introductory U.S. politics course, for it puts flesh upon Tocqueville's concluding paragraph of *Democracy in America,* which warned against the kind of pseudoscientific determinism that enfeebles men by excusing their faint-heartedness. "I am aware," wrote Tocqueville in 1840, "that many of my contemporaries maintain that nations are never their own masters here below, and that they necessarily obey some insurmountable and unintelligent power, arising from anterior events, from their race, or from the soil and climate of their country. Such principles are false and cowardly; such principles can never produce aught but feeble men and pusillanimous nations. Providence has not created mankind entirely independent or entirely free. It is true that around every man a fatal circle is traced beyond which he cannot pass; but within the wide verge of that circle he is powerful and free; as it is with man, so with communities. The nations of our time cannot prevent the conditions of men from becoming equal, but it depends upon themselves whether the principle of equality is to lead them to servitude or freedom, to knowledge or barbarism, to prosperity or wretchedness." *Governing Prisons* honors those words devoutly.

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Telecommunications in Turmoil: Technology and Public Policy. By Gerald R. Faulhaber. Cambridge, MA: Ballenger, 1987. 186p. $26.95.

Teledemocracy: Can Technology Protect Democracy? By F. Christopher Arterton. Newbury Park, CA: Sage, 1987. 224p. $14.95.

As the "information age" or postindustrial era grows beyond infancy, inseparable interrelationships between communications and politics become all the more blatant. Consequently, a cadre of scholars has emerged, creating subfields in various disciplines known as "media and politics," "political communication," and so on. Some members of this corps of intellectuals are unabashed advocates of ideological or policy positions. Others approach the subject from what they claim is a strictly professional, detached, or even scientific point of view.

The two recent works under review are cases in point. Faulhaber's *Telecommunications in Turmoil* is a clever legal brief for an extreme viewpoint. Arterton's *Teledemocracy*, taken at face value, is a scholarly analysis of a number of innovative field experiments. However, both of these books are themselves vivid illustrations of the interface between communications and politics. Both are political tracts with definite agendas. The difference is that one's agenda is ostensibly clear, while the other's is opaque. At the heart of each is a politically conservative message.

Let's start with Faulhaber, currently a professor at the University of Pennsylvania's Wharton School. It is neither irrelevant nor immaterial to observe that he was previously a "director of strategic planning and financial management at American Telephone and Telegraph" and prior to that held the job of research head at Bell Labs. Thus it is not too surprising to learn that the "turmoil" in the telecommunications industry of which he speaks is due solely to government interference, that is, federal regulation.

Although not an avowed libertarian, Faulhaber's argument will win the minds of Adam Smith devotees and the hearts of Ayn Rand purists. What he wants is a total and abject government withdrawal from anything to do with the communications industry: "What we need is to rid telecommunications of regulation—not 'regulatory reform,' not 'managed transition,' and surely not 'managed competition' " (p. 164). The only way this can be done is through an act of Congress and only through this complete legislative retreat to the nineteenth century can "everyone be a winner" (p. 165).

How can Faulhaber be so sure this will work? He uses no tables, charts, trend analyses, aggregate data, futuristic forecasting techniques, crystal ball, or divining rod. What he does have is the power of example. Unfortunately for him, the one he uses is the deregulation of the airline industry.

From his secure and lofty perch as an industry-efficient analyst, the author applauds today's "efficient hub-and-spoke" airline operations, with airline seats full of passengers and lower salaries for pilots. By following this exemplary model, he predicts that the U.S. telecommunications industry can provide us, one and all, with a functional equivalent of the "better service" (pp. 166–67) the airline companies have demonstrated since deregulation.

Perhaps Faulhaber travels exclusively by train. Perchance his air jaunts are confined to AT&T corporate jets. But as a commercial airlines tourist class passenger myself, I tremble to think how a future telecommunications industry's "better service" would equal the ever-narrowing seats, ever-longer delays, increasingly lost, misrouted or brutalized luggage, and the frequently missed connections that have accompanied—if not characterized—the deregulation of the U.S. airlines industry.

Thus what is billed as—and first appears to be—an examination of communications "policy" turns out to be an elegant, albeit transparent, polemic on one of the fundamental system-process issues of our time: state capitalism or statism versus unbridled laissez-faire. Communications policy is merely the focus.

F. Christopher Arterton's book, *Teledemocracy*, also deals with system-process change, although in style and substance it is mildly conservative instead of wildly reactionary. The stated purpose of his brief treatise is to investigate 13 "policy-neutral" U.S. teledemocratic projects in terms of how well they increased citizen participation (and "protected democracy"?). The author constructs a framework of 11 criteria to evaluate the degrees of success or failure of each project.

Arterton derives these standards from a summary and analysis of the continuing debate on the (in)effectiveness of contemporary U.S. democracy that he sketches artfully in his first chapters. These criteria include (1) "the range of citizens able to participate"; (2) "whether or not citizen participation [had] a direct influence on public policy"; and (3) "the burdens, financial and otherwise, imposed on citizens in connection with their participation" (p. 63). Then Arterton classifies and describes each project and subjects them to a close scrutiny against these 11 criteria. Each is found to be seriously deficient on a number of dimensions. Many are judged by Arterton to be "disappointing" or to fall "far short" of his goals.

This is not to say that the author had nothing good to say about any of the projects. Indeed, he finds at least as much good in them as bad and concludes that teledemocracy offers a promise of "slight" or "modest" improvements to the present system of U.S. democracy (p. 204).

So what's conservative about that? How can this staid and temperate empirical inquiry be considered in the same league as Faulhaber's overtly political tome? Just as reputedly scientific public opinion polls may be constructed to bias their results, so may apparently balanced academic investigations produce, ever so subtly, an expected or preordained result. This can be accomplished by the research design itself or through its execution. For example, suppose an academic researcher was funded to study experimental theatre in the United States. Instead of a research protocol that probed, systematically and in depth, the views and impact of these experiments on the writers, directors, actors, critics, and audience, suppose that the design (1) compared the number of people who saw Broadway shows and movies to those who attended experimental dramas and (2) speculated subjectively about the effect of these experiments on mainstream theatre. The inevitable conclusion: the consequence and promise of experimental drama are disappointing. This is precisely what Arterton has done.

Next Arterton imposed an onerous and strict ex post facto set of evaluative criteria on the experimenters but, oddly enough, failed to be rigorous in his own system of evaluation. For example, in noting that one project had no effect on public officials, he based his conclusion on two interviews of nonelected officials (p. 91). In verifying the "reach" of another project, he conducted a random survey of only seventy people (p. 123).

Arterton may not have *consciously* intended to convey the impression that these projects did not accomplish much, but his book damns by faint praise teledemocracy's promise for the future of U.S. politics. Unfortunately, that is often the latent function of academic research in the United States: to criticize and analyze something to death, particularly if it involves theory or field research that may lead to a radical restructuring (democratization) of the political system. The result is part of an old story—an academic establishment, comfortable and cynical and fundamentally at odds with those who struggle in the "real" world to solve systemic problems that the very same academics concede to be serious.

And that is status quo—conservative—politics, no matter what the rhetoric.

TED BECKER

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Referendum Voting: Social Status and Policy Preferences. By Harlan Hahn and Sheldon Kamieniecki. Westport, CT: Greenwood, 1987. 150p. $35.00.

This short book is a welcome addition to the limited literature on municipal referenda and focuses upon the impact of social class—as measured by housing values—on voter decisions. Thirty-eight nonfinancial referenda in 15 U.S. cities and 14 similar referenda in 5 Canadian cities between 1955 and 1970 were examined. In addition, 228 tax-and-expenditure referenda in 29 cities during the same time period were analyzed.

The principal focal point is an evaluation of the theory of "decreasing scarce resources," which posits that high-status voters may "support relatively expensive policy proposals consistent with the amenities that they wish to enjoy, while the obverse—or 'increasing scarce resources'—may provoke similar patterns among lower-status voters, with implications for the increasingly constricted support of community programs" (p. 47).

Examined referenda included legalization of gambling, water supply fluoridation, blue laws, a municipal franchise, and a property requirement for voting. The authors reported that higher-status groups do not support the legalization of gambling but appear to have "greater tolerance for pornography" (p. 95) and favored sporting event regulation and daylight savings time. The findings relative to nonfinancial propositions are interpreted as a challenge to the "public-regardingness" theory, which earlier had been dismissed as an inadequate explanatory theory and support for the "decreasing scarce resources" theory.

Examination of referenda in 29 cities generally supported the findings of earlier studies that higher-status voters tend to approve financial proposals. Wealthier voters are assumed not to be concerned about making a choice between different spending proposals since the additional costs they will bear are minor relative to their resources. The authors maintained the type of tax was not a major concern to the wealthy voters.

Hahn and Kamieniecki also concluded that high-status voters "are not prepared to subsidize the disadvantaged" in referenda on health and welfare propositions and suggested the reason for this finding is that high-status voters have a different view of "fundamental economic values" and the manner "in which a person achieves social status in American economic life" (p. 124).

The structure of a city government—reformed or nonreformed as measured by an index of reformism—is identified as a major variable with status affecting the vote to a lesser extent on expenditure and tax issues in reformed cities. The authors noted that the tax referenda were held chiefly in the decade of the 1960s, "when there may have been somewhat less concern over the issue of tax limitation than in the 1970s" (p. 123).

Readers are advised that care should be exercised in drawing conclusions about individual voting behavior from aggregate data and interpreting the data collected in the period 1955–70. Relative to the latter, they are confident that the voting propensities of the electorate on referenda issues, as reported in their study, are valid.

Questions can be raised as to whether the referenda results are valid only for the time period in which the referenda were held. Although the wealthy voters were found not to be concerned about the type of tax, congressional ending of the federal income tax deduction for state and local sales taxes, effective in 1987, may have affected the attitude of these voters toward the levying or increasing of the rate of a sales tax. The authors also failed to mention the influence on referenda results of the 1978 U.S. Supreme Court decision in *First National Bank* v. *Bellotti*, which struck down provisions in state corrupt practices acts prohibiting corporations from contributing to referenda campaigns if corporate interests were involved.

The findings relative to the influence of reformed structure on referenda results would have been strengthened if evidence had been provided relative to the presence or absence of an active citizen association supporting or opposing propositions.

The weakest chapter is the concluding one, which is little more than a rehashing of findings and a lamenting of the absence of redistributive propositions on the referendum ballots. Briefly discussed are "some possible reforms," including the use of surveys to determine which issues should be placed on the ballot to avoid overburdening the electorate, postal registration of voters, public financing of referenda campaigns, more effective enforcement of spending limits, and judicial evaluation of propositions prior to their placement on the ballot.

The study would have been of considerably more value had a comparative focus been employed with results from the 1955–70 period contrasted with the results of post-1978 referenda. In addition, the book would be better if it contained an expanded methodology section or a methodological appendix and listed all cities visited to obtain information.

The above critical comments do not detract from the value of this book, which is the first to examine in a systematic manner the relationship between the votes cast in referenda and social class. The authors succeeded in their avowed goal of resurrecting to prominence the social status theory of voting.

JOSEPH F. ZIMMERMAN

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Book Reviews: American Politics

A Slippery Slope: The Long Road to the Breakup of AT&T. By Fred W. Henck and Bernard Strassburg. Westport, CT: Greenwood, 1988. 288p. $37.95.

The Fall of the Bell System. By Peter Temin with Louis Galambos. Cambridge: Cambridge University Press, 1987. 378p. $27.95.

Two new books on the 1984 AT&T divestiture deserve special notice among those on the industry. While both ring with great authority, one was written and fostered by participants in the long trail leading to the breakup. The other was a "commissioned work."

A Slippery Slope: The Long Road to the Breakup of AT&T, is probably the better of the pair. It was written by Fred W. Henck, longtime editor and publisher of the now-54-year-old *Telecommunications Reports*, and Bernard Strassburg, who for 10 years was chief of the FCC's Common Carrier Bureau. While journalist and regulator provide joint accounts in most of the 288 pages, each treats some events and trends in notations denoting individual views. The other key work on the breakup is *The Fall of the Bell System*, by Peter Temin with editing by Louis Galambos. Temin is an MIT economics professor. Galambos, a history professor at John Hopkins, had earlier edited the papers of Dwight D. Eisenhower. The Temin-Galambos book originated in the desire of AT&T Chairman C. L. Brown "to commission a scholarly study of the breakup of the Bell System, a book that would serve as a point of reference for other studies." Brown got what he wanted.

Both works end with reflective sections in effect viewing the future with nostalgia. The authors recognize that the divestiture was an inevitable move to a new way of doing telecommunications business, one that is far less comfortable than the "Pax Americana," which existed for so many years in the industry. Why indeed did the breakup occur? Telephone service had been the world's envy, efficient, affordable, and ubiquitous. Before and after the Bell System had overcome its late-1960s run of service problems (due to poor demand forecasts), complaint levels were low. Both books credibly develop the theme that the new (some say contrived) competition in the industry was unable to coexist with regulation.

Henck and Strassburg correctly trace the breakup to its origins 50 years earlier—at passage of the Communications Act in 1934. The next year, the new FCC launched the telephone investigation and gave the final report to Congress in 1939. A decade later, the Justice Department used that report as the basis for its antitrust suit asking breakup of the system.

Strassburg's "Outlook" section sees signs of returning to monopoly or perhaps cartelized oligopoly status in the long distance business because duplication via multiple suppliers is at odds with economies of scale and the general public interest. Given Strassburg's dominant place in the lineage of competition in the industry, such a publicly uttered projection is surprising. He clearly expresses his doubts that the consumer is benefiting from the competitive status of the industry.

Henck's epilog notes, "In telecommunications, those best equipped to look out for themselves have benefited from deregulation and competition" and observes that "wide open competition does not seem to be in the cards." Henck sees the critical question for the future as "whether the nation's telephone customers will benefit, not just whether the competitors can survive."

Besides the advent of competitive offerings of equipment and services, authors Henck and Strassburg identify and track five key sources of the breakup of the system. First was the FCC, "almost always the focal or departure point of the policy changes that have come about." Next was Congress, with its "amorphous role," which served "as a galvanizing agent in compromise policy changes administratively fashioned by the FCC and acceptable to diverse forces which could not agree on the precise terms of legislation."

Third were numerous federal judges "who did more to influence the direction of government policy than either the FCC or Congress," namely, by causing changes in FCC decisions and hearing the many lawsuits. Fourth on the list came "a militant group of anti-trust specialists in the Department of Justice who concluded that the time had come to restructure the communications industry more in keeping with a competitive environment." Finally, "the responses of the industry itself were critical to the outcome. The Bell System often chose not to yield any part of its monopoly domain to the new entrants or to adjust its policies to the advent of a competitive marketplace. Eventually, in the face of the forces arrayed against it, this led to its dissolution." Intransigence, resistance to change and the relish for noisy confrontations on the part of some high AT&T officials are well documented by Henck and Strassburg.

The Temin book concludes by generally citing the same causes leading to the breakup of the system. First was the advent and growth of competition in the industry. Second was the pervasive change in ideology dictating both the government's decisions on competitive entry and AT&T's choices on its internal structure. Third was that implementing change was greatly complicated by the system's internal culture, the legacy of Theodore N. Vail. The book explores the system's "rigidity . . . which hurt AT&T in the political process."

Temin notes that while the public policy and corporate strategy implications of divestiture cannot yet be evaluated, some strong indications have come forth: "[Former chairman Charles L. Brown's] twin aims of protection of the stockholders' interest and the maintenance of service seem to have been largely accomplished." The person holding AT&T stock gained appreciably by keeping everything. Business customers found the range of services increased but residential users found the quality of their repair and installation services impaired. AT&T employees, as indicated by the number of layoffs and strikes, did not fare so well. Each of these points is well developed in Temin's work.

He ends the book by observing that the breakup must be viewed as "an enormous gamble," namely, in organizing a previously regulated and unique industry.

Because Temin had full access to AT&T documents and persons in and out of the company, including *A Slippery Slope*'s coauthor, Bernard Strassburg, the account of the breakup reads as an insider's story. This book more than adequately footnotes the interviews and primary sources for treatment of the many events and cases leading to the breakup.

JOHN MORGAN

Communications Workers of America

If I Had a Hammer—The Death of the Old Left and the Birth of the New Left. By Maurice Isserman. New York: Basic Books, 1987. 259p. $18.95.

Social scientists, historians, and political actors have long been asked why there is no socialism in the United States. Most of the literature that has been produced in pursuit of the answer to this question focuses on structural explanations having to do with the composition, dispositions, and organization of the U.S. working class, with the nature of the U.S. political system (widespread suffrage, federalism, localism, civil liberties, etc.), with the attractions of upward mobility in U.S. capitalism, or with the hegemony of a political culture rooted in the individualism of classical liberalism. Isserman, while aware of this literature, seeks to locate much of the failure of Left politics in the United States in a series of wrong strategic choices and other mistakes of commission and omission on the part of the principal organizations of the Left.

Isserman's major focus in this endlessly fascinating book is on the myriad ways in which the rise of the New Left in the sixties was rooted in the organizations, personalities, debates, and ideas of the Old Left. In the course of this scholarly journey, he demonstrates how the 1950s were not entirely a time of consensus, one described by C. Wright Mills as "the great American celebration," but was rather a period with strong currents of Left opposition that would flourish in later years. Isserman also convincingly demonstrates that the New Left was never entirely new, as often assumed by those most responsible for the intellectual and organizational construction of the New Left; for while much of the Old Left had disappeared by the late 1950s, remnants carried on through the darkest days of the cold war and contributed tangibly to the construction of a New Left. In passing on its legacy, however, Isserman argues that the old saddled the new with many of its traditional problems, while the new failed to learn properly from the bitter dead-end experiences of the old. The great strength of Isserman's book is in the rich detail of the telling of this complex story. In its pages we learn of the many and even heroic efforts of Communist party reformers, Shachtmanites, the intellectuals around *Dissent*, socialist campus organizations, and the peace movement during the fifties to define an alternative vision of the United States.

If detail is the strength of the book, the weakness is in Isserman's failure to move beyond detail and to tie his rich historical description to a structural analysis of the United States during the fifties and sixties. While he occasionally touches base with structural issues, he does not take the impressive structuralist literature on the state of the Left seriously enough or try to conduct a dialogue between this literature and the details of individual and organizational action that constitute most of his book. One is left with a story that, in my view, is much too "volunteerist." In the course of making the case that choices by Left organizations about strategy mattered, he leaves the impression that it was virtually all that mattered, that had Left organizations only made the correct strategic choices, a vital and important movement of the Left could have been fashioned. In so doing, while telling a fascinating tale, the work remains unconvincing in the end, for it fails to consider very seriously what was possible in a structural sense. I remain unpersuaded, that is, that the outcome of the story (namely, the decline of the Old Left and the rapid rise and fall of the New Left) would have been much different in light of better strategic choices. The failure of the Left in the United States, in my view, is better located in issues of class structure, cultural hegemony, and the nature of the capitalist state.

EDWARD S. GREENBERG

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Black Political Mobilization: Leadership, Power, and Mass Behavior. By Minion K. C. Morrison. Albany: SUNY Press, 1987. 303p. $49.50 cloth, $19.95 paper.

Whose Votes Count? Affirmative Action and Minority Voting Rights. By Abigail M. Thernstrom. Cambridge, MA: Harvard University Press, 1987. 316p. $25.00.

These books are valuable contributions to the growing literature on black U.S. politics. *Black Political Mobilization* draws most of its data from three small towns in rural Mississippi, but the author suggests that the recent political history of these communities is representative of communities throughout the South where blacks are well represented in the electorate. *Whose Votes Count?* is a serious attempt to assess the impact of the Voting Rights Act of 1965 and some of its key amendments.

The Morrison book begins with critiques of pluralist and elite theories of democracy. The author goes on to suggest a new eclectically derived mobilization model based on an appreciation for the methods that blacks were forced to use in order to make U.S. democracy work for them. Morrison argues that black mobilization in Mississippi after World War II was analogous to nationalist movements in the third world. This mobilization culminated in the 1970s, with a virtually complete transfer of institutional power to blacks in those communities where they had gained political consciousness and were guided in their political objectives by visionary leadership and a vigorous strategic organization.

The towns in this study were Tchula (population 1,739), Bolton (population 816), and Mayersville (population 500). Morrison couches his analysis first in the context of historical black political participation in the South and then hones in on the communities under study. The author does an excellent job of describing black political consciousness from the slave era to the present. He focuses on three small towns that were characterized by heightened political consciousness in the civil rights era among a numerically dominant black population, who in turn were led by youthful, educated, charismatic, interventionist local black leaders. The movements that resulted led to blacks gaining local political power for the first time.

Although this book is based on a very limited number of survey interviews, Morrison's contribution to the literature lies in his penetrating analysis of the context of present-day life and politics in rural Mississippi. He points out that merely gaining local political office is not enough for blacks. As a group, they continue to be poor, economically weak, and vulnerable. This gives rise to a dependency syndrome that will afflict black communities for some time to come. Morrison's study shows that black political mobilization has its limits and that blacks remain only on the fringe of national ruling coalitions.

Although he does not specifically say that the next stage of the black struggle is largely a class struggle, this implication is evident from the analysis. This book makes a timely contribution to the literature on black political participation.

In contrast to the Morrison book, *Whose Votes Count?* is grounded in an analysis of the Voting Rights Act and numerous court cases that have shaped the character of voting rights law over the past two decades. Thernstrom does a fine job of tracing the history of the Voting Rights Act and spelling out the various revisions of the law. Her main purpose, however, is not merely to document how blacks and Latinos have increased their voting power. She also wants to question whether the Congress and the courts have gone too far in guarding against race inequality in the electoral system. She asks, "How much special protection from white competition are black candidates entitled to?" "Are blacks and Hispanics entitled to safe minority seats reflecting their proportion of the population?" "Are minorities better off with fewer seats, but more diffuse influence?" Thernstrom contends that the true intent of the Civil Rights Act has become obscure to federal legislators and judges over the years. It was designed only to ensure equal access to the ballot, not to guarantee a certain proportion of minority representation in government. The author argues that the 1982 revision of the statute has given minority voters unprecedented power to insist on methods of voting that facilitate minority office holding. At-large voting arrangements are fast disappearing. Rather than seeing this as beneficial to minorities in the long run, Thernstrom maintains that this will only lead to further racial polarization. She advocates political integration and biracial coalitions instead of solid minority voting blocks. The author chides civil rights organizations for not seeming to believe that a race-neutral "community of citizens" is possible.

Black and Latino activists and scholars alike would find much to criticize in this book. The facts cannot be quarreled with, but the author's interpretations are open to question. The book's tone is one that suggests that there is no longer a need for the close monitoring of the enforcement of the Civil Rights Act. However, evidence to the contrary can be found in the very pages of this book! Yet the book has value in that it contributes to the affirmative action debate.

EDMOND J. KELLER

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Race and Ethnicity in Chicago Politics: A Reexamination of Pluralist Theory. By Dianne M. Pinderhughes. Urbana: University of Illinois Press, 1987. 318p. $27.95.

Chicago once again provides the municipal setting for an examination in this book of the politics of race, ethnicity, and the political integration of previously unrepresented groups. The study incorporates the rich body of prior scholarship on Chicago politics and contributes important original research to that scholarly tradition. Pinderhughes' central argument is straight forward: race and ethnicity are not of equal status in U.S. city politics and the differences that define race from ethnicity retard the ease, speed, and degree of political inclusion of racial groups as compared to ethnic groups. Barriers to racial inclusion are identified throughout to account for the subordinate status of Chicago's blacks. The case of black exclusion is then developed critically to assess arguments about the penetrability and openness of the political system as advanced in pluralist theory.

Between the introductory and concluding chapters, the core of the book focuses on the time period 1910–40, comparing the black racial experience to the Italian and Polish ethnic experiences. Pinderhughes selected the two immigrant ethnic groups for study because their arrival in Chicago closely corresponded to black migration to Chicago. In chapters 2 and 3 the author shows how business and labor markets facilitated ethnic but not racial advancement, how housing markets concentrated blacks residentially more than the ethnic groups, and how the evolving Democratic political machine differentially impinged on the three communities. A model of political participation is introduced in chapter 4 to predict group success in winning citywide political leadership positions. When the model is applied, blacks are found politically dominant in areas where they were concentrated, but—unlike the ethnic immigrant groups—their representation stopped there. Chapter 5 develops an argument for a distinctive black political ideology that derived from the experience of racial discrimination and produced identification among blacks with collective racial status issues. Chapters 6 and 7 conclude the core of the book by examining the criminal justice and educational policy areas to assess the varying policy responsiveness of the political system to the three communities. Pinderhughes shows black underrepresentation and argues that municipal policies were unresponsive to blacks while higher inclusion rates were found for Italians and Poles, who also received more favorable policy treatment.

Throughout the central section the author provides several important insights about ethnic and racial community politics. The discussion of community characteristics distinctively influencing neighborhood organizations, leadership structures, and community institutions is richly textured. The evolution from competitive party structures in the 1920s, where blacks had political voice, to a monopoly structure in the form of the Democratic machine, where black influence flagged, is amply documented and effectively tied into differential policy treatment blacks received. The model of political participation persuasively suggests the constraints imposed on internal community organizations by external environments. A powerful argument about the conditions associated with high black electoral participation, historically and contemporarily, is presented when Pinderhughes suggests that since race is the single most important factor affecting blacks, in those elections where race is an issue or black candidates are on the ballot black turnout is unusually high, often exceeding that of whites.

Events taking place before 1910 and after 1940 are treated less thoroughly. A brief introductory chapter discusses the demise of the Irish-dominated machine in the 1970s and the subsequent black succession to power in Harold Washington's successful 1983 mayoral bid. The final chapter offers an explanation for the black electoral triumph of the 1980s, which is less convincing, perhaps due to how close the author was to the events themselves. Thus incompetent leadership is a recurring explanation for the demise of the machine, and the analysis of Mayor Washington's organizational base is less well developed than the earlier, historical, organizational analysis. Perhaps more important, black succession to power has important implications for the continuing debate over pluralist theory that could be more fully developed. These reservations invite continuing scholarly research on processes of racial exclusion and inclusion, to which Pinderhughes has made an important contribution.

There is much in this book to recommend it to a variety of scholarly audiences. Students of urban studies will find in it a useful complement to the already vast literature on Chicago politics. Those interested in race and ethnicity will find the comparisons and contrasts between European immigrant groups and blacks of interest. The book is sure to occupy an important place in contemporary black studies. The discussion of criminal justice and education policy responsiveness will interest students of public policy. Pluralist theorists will find in Pinderhughes' book yet another critique about how pluralism fails to account for the case of racial minorities.

DAVID J. OLSON

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A Republic, If You Can Keep It: The Foundation of the American Presidency, 1700–1800. By Michael P. Riccards. New York: Greenwood, 1987. 227p. $35.00.

First Use of Nuclear Weapons: Under the Constitution, Who Decides? Edited by Peter Raven-Hansen. New York: Greenwood, 1987. 249p. $37.95.

The Secret Constitution and the Need for Constitutional Change. By Arthur S. Miller. New York: Greenwood, 1987. 179p. $29.95.

As we head into the third century of constitutional government, these three books help light the way. One looks back to the creation of the presidency; another examines the most portentious problem of our own time, the control of nuclear weapons; and the third assesses the "survivability" of the constitutional system in the face of future challenges.

Michael Riccards takes the title of his book from Benjamin Franklin's remark as he departed from the convention of 1787. Riccards' book is laid out in two parts. The first, 50 pages long, presents the roots of the U.S. tradition about executive power: the ideas of Locke and Blackstone, the experience with provincial governors and with U.S. leadership during the Revolutionary War, and the framing and ratification struggles. Part 2—about 150 pages long—surveys the history of George Washington's two terms as president.

Those who turn to this book for clarification about what Franklin and his colleagues may have meant by republicanism and whether we have "kept" it will be disappointed. Part 1 is superficial and textbookish and seems oblivious of the best recent scholarship on the colonial and early national periods.

Part 2 is old-fashioned narrative history. It is full of good yarns. But it must be read warily, for it is also full of errors. For example, there is a reference to "differential politics" (for *deferential*, p. 62), and Washington's unsuccessful Supreme Court nominee is cited as "John Randolph of Virginia" instead of John Rutledge of South Carolina (p. 113).

On the other hand, the book has some real strengths. I found the chapter on Washington's policy toward the Indians fascinating and full of relevance to modern concerns. Right from the start, it seems, the executive branch fought with Congress over the defense budget and the right to expose ill-fated policies to public scrutiny. One gains a deeper respect for the first president as, with the surprisingly able assistance of his secretary of war, Henry Knox, he groped to frame a policy amidst uncertain constitutional provisions and a maelstrom of racist passions.

The book contains at least one noteworthy interpretive idea: the suggestion, offered in the introduction (p. xiii), that Washington created a "model of the presidency" that fell into disrepute during the nineteenth century and has been "ironically rediscovered in the latter part of this century." Its main features, writes Riccards, are "mass appeal," strong executive authority, "general disregard of political parties," attention to pomp and protocol as means to buttress the incumbent, conservatism on economic and social matters, and "aloofness toward legislative controversies." Riccards contrasts this model with Jefferson's "party government [and] cabinet leadership" and with Jackson's populism, which "delighted in confrontations with Congress and with various elites." With the recent breakdown of the party system and the longing for presidential stewardship based on a sense of personal communication with the people, Washington's "model," suggests Riccards, is again ascendant. Sadly, he never mentions this idea again after the introduction, though it seems to me worth pursuing.

The second volume edited by Peter Raven-Hansen makes a solid contribution to the debate about the viability of constitutional guarantees under modern conditions. It presents papers given in November 1985 at a symposium sponsored by the Federation of American Scientists and the Lawyers Alliance for Nuclear Arms Control addressing a proposal that Congress enact a ban on the first use of nuclear weapons by the United States, except in a declared war, to be waived only with the assent of a congressional leadership group.

The volume contains 11 papers: four that discuss the allocation of war powers under the Constitution, three on the form that congressional participation might constitutionally take, and four on problems of implementation. (Can Congress be trusted with sensitive national security data? Could the courts force presidents to obey this law?) Most of the authors are professors of law. A few have served on congressional staffs.

The problem confronted in this volume is a terrible one. The Constitution draws a reasonably clear line between the legislative and executive responsibilities in war making. Congress raises troops and determines the need to go to war; the commander-in-chief conducts operations.

Modernity has changed that. With standing armies and a worldwide network of entangling alliances, presidents have the means and authority to wage war on their own. And with launching mechanisms—our own and our adversaries'—on perpetual alert, our national security demands that the president—who alone can act instantaneously—have the authority, when necessary, to do so.

This book addresses two dimensions of the modern dilemma. The first is the nuclear threshold. Ought presidents to have the authority to initiate the use of nuclear weapons? Is that a command decision, once Congress has provided them and we are lawfully involved in hostilities? Or is it a policy question of sufficient gravity that Congress may and should by law forbid presidents to use these weapons without its explicit consent?

Assuming that the case can be made that congressional participation ought to be required, the second part of the dilemma has to do with the proper mode. It is impractical for 535 men and women, in two chambers, to take counsel with the president in the heat of hostilities about whether a particular emergency justifies the use of our nuclear arsenal. After the *Chadha* decision, can Congress lawfully empower a committee to act on its behalf? Will presidents accede to this arrangement? If not, can anyone force them to do so?

The essays in this book are carefully argued and thoroughly documented, and they represent a wide range of scholarly opinion. Particularly valuable were the judicious and skeptical analysis of the Federation of American Scientists proposals by John Norton Moore; Michael Glennon's review of the contention that treaties like NATO commit U.S. presidents to defend Copenhagen as they are committed to defend Staten Island; and William G. Miller's well-informed defense of Congress against the charge that it is not to be trusted on matters of national security. The editing by Raven-Hansen is also commendable, as are his succinct introduction and conclusion.

The third book, by Arthur S. Miller, presents a vigorous call for constitutional change. As Miller himself notes several times, it is meant to be suggestive rather than exhaustive.

Miller contends that there are in reality two constitutions in the United States: the formal, written one, and a "secret," unwritten one, an operational code of informal understandings and conventions. The former he disparages. It is essentially a myth, whose principal function is to mask what happens in accordance with the latter. The unwritten constitution legitimates groups, virtually ignores individual persons, and grants special privileges to supercorporations.

This system of governance has worked for two centuries owing primarily to the continent's abundance. But time is running out. The tide is rising, and "the natives are restless." We are on the brink of a "societal nervous breakdown." We must move soon toward a "sustainable society," one that grants all people what they need and deserve, in accordance with the limits on population and resources that nature prescribes.

To do this, we must change the Constitution—not so much its structural features as its definition of governmental powers. We must make it clear that Congress has power to make and enforce measures to achieve an "optimum population," to "defuse the threat of nuclear war," and to control environmental degradation. In addition, Congress must have power to "check the excessive use of presidential and bureaucratic power," and the Supreme Court must have original jurisdiction to determine the validity of allegations that Congress has failed to perform any of these new duties. For this purpose, "any citizen eligible to vote . . . shall have a right to bring an action in the Supreme Court, the writ of which, if granted, shall run against Congress generally and the members specifically."

Structural changes are given short shrift, though not ignored completely. Miller suggests that Congress be reduced to a single chamber with one hundred members, that the presidency be "bifurcated," that political parties be given control over members of Congress, that the 50 states be "telescoped" into regional governments, and that the "judicial role . . . be expanded."

Miller's ideas are obviously "utopian," as he freely admits. The question is, What value do they have? His diagnosis of the challenges we face is very shrill. Drawing heavily on the work of Lester Brown, Paul Ehrlich, and other like-minded folk, he paints an apocalyptic picture of the human future. The trouble is that his remedies seem almost bizarre, and he disdains to explain or defend them.

We should resist the easy impulse to dismiss proposals for constitutional change. The bicentennial provided very little opportunity for a broad public discussion of the fitness of our constitutional system for the challenges of its third century. However, the Iran-Contra hearings, the battle over the nomination of Robert Bork, and the collapse of the stock market vividly revealed strains in our system and raised the level of public discourse on these vital matters. It is a discussion that I hope political scientists will want to join.

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The Development of American Federalism. By William H. Riker. Boston: Kluwer Academic, 1987. 233p. $38.95.

William H. Riker has reissued here a body of work on federalism written over a 30-year period. Eight of the 11 essays in the book were published before 1970. One essay is a previously unpublished 1985 paper on the invention of centralized federalism by the framers of the U.S. Constitution. Two essays represent previously unpublished work by two of Riker's former graduate students. Hence, most of the essays are familiar to veteran students of federalism.

The book, therefore, seems aimed more at young scholars, those trained mainly in the tradition of federalism as intergovernmental relations. In this respect, the book is a valuable reiteration of important issues in federal theory. Riker, a pioneer in the study of federalism, is hardly concerned with intergovernmental relations. Instead, he approaches federalism as a mode of political organization lying, in his view, between alliance and empire. Riker therefore addresses the structural politics of federalism: how federal systems come into being, how they operate as a complex of central and peripheral political institutions, and how they are maintained, especially with respect to centralization and decentralization.

Intellectually, though, Riker is largely preoccupied with political issues of U.S. federalism that dominated the liberal reform agenda of his generation. Methodologically, he is preoccupied with the tools of behavioral inquiry that were being developed during the 1950s and 1960s. These two preoccupations are joined in the book's central concern: the maintenance of a steady-state process of centralized federalism.

In a personal introduction to the book, however, Riker reverses his 1964 view that U.S. federalism is little more than a device for perpetuating racism. This reversal, he says, is associated with civil rights reforms and with his own ideological migration "from New Dealer in the fifties to liberal, anti-statist in the eighties." As a former New Dealer in the Reagan era, Riker now concludes that federalism is "a desirable, though still minor, restraint on the leviathan."

Nevertheless, an overriding concern for Riker is the legitimation of centralization. He has no wish to "strengthen the central government so much that it can cannibalize the constituents," but maintenance of a strong, centralized federal system is essential. Riker seeks to legitimize centralization by arguing that U.S. federalism was centralized from the start. Compared to the Articles of Confederation, the U.S. Constitution established a highly centralized, though not unitary, polity. In certain respects, this point seems obvious. However, it becomes problematic when Riker tries to show a fundamental continuity of centralized federalism for nearly two hundred years. In short, the New Deal was not so new.

In the new essay published here, Riker examines key federalism debates in the Constitutional Convention of 1787. Although nationalists, such as James Madison, got less than they initially wanted (e.g., no congressional veto of state legislation), they did, in Riker's view, push federalism largely into the margins of the Constitution. One of the federal devices that made it to the center of the Constitution—state legislative selection of senators—failed to fulfill its purpose, argues Riker, and was excised in 1913.

This is a curious essay, however. It was originally presented at Indiana University for Vincent Ostrom's inauguration as Bentley professor of political science. Riker does not mention Ostrom's arguments set forth in *The Political Theory of a Compound Republic,* which was revised and reissued in 1987. In addition, no attention is given to new scholarship on the founding period. Nor does Riker attend to the ratification debates and, thereby, to what the people may have understood themselves to be ratifying in the state conventions. No attention is given to the antifederalist insistence on a bill of rights to further protect both persons and the states against tyranny and centralization.

Riker understands federalism to be a constitutional bargain, but he looks at the nationalist view of the bargain and at only one piece of the bargain, namely, the first, unamended offer put on the table. With respect to the first offer, moreover, he looks mainly at its structural and procedural provisions. Scant attention is given to the substantive provisions of the Constitution that created a "general government" (Madison's term) possessing limited, delegated powers, not a "central government" (Riker's term) possessing virtually plenary powers.

By finding centralization in the birth of U.S. federalism in 1787, Riker can conclude that "a more or less steady state has continued since then"—although there has been "intense centralization of effort and expenditures" as well as "disharmony." Two essays, for example, make use of a crude historical index of political disharmony in the federal system. Amazingly, the index shows decreases in disharmony during the Civil War era (1861–73), the early New Deal years (1933–37), and the era of massive southern resistance to the national assault on radical segregation (1959–65).

Primarily, according to Riker, what prevents the federal system from becoming completely centralized and thereby unitary is the inability of national officials to control the nation's partisan political process. In particular, both the separation of powers within the national government and the decentralized structure of the political parties act as breaks on centralization. This does not mean that states can control national decisions, but it does mean "that the nation cannot control state decisions. The result is a standoff, which is . . . intended in the federal bargain." Riker seems to anticipate the views expressed by the majority of the United States Supreme Court in *Garcia* v. *San Antonio Metropolitan Transit Authority* (1985) and *South Carolina* v. *Baker* (1988); that is, state prerogatives are protected well enough by the political process, and there is no need to activate the Tenth Amendment as a break on centralization.

There is much that is rich and rewarding in this book, and some of the theses for which Riker is well known, such as the military and expansionist origins of federalism, deserve careful scrutiny. Yet the issue that dominates the book and the methods used to substantiate the steady-state thesis give this book the character of a historical footnote, albeit an important footnote that helps to show how we got where we are today in both federalism and political science. In light of the nationalizing and internationalizing forces at work in U.S. society, however, plus abuses of power by the no-longer-so-liberal national government, one wonders how much solace an antistatist can find in a federalism whose constitutional status has been reduced to that of a contest of wills.

JOHN KINCAID

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Hispanics in American Politics: The Search for Political Power. Maurilio E. Vigil. New York: University Press of America, 1987. 147p. $23.50 cloth, $11.75 paper.

Maurilio Vigil's latest book is one of a handful of scholarly works on Latino politics in the United States and one of still fewer that focuses on Latinos in the electoral arena. He calls his "a first work on Hispanics in American politics," and to the extent to which he characterizes individual Latino electoral success stories and calls for Hispanic political homogeneity, it probably is. However, this is not the first work on Hispanic-Latino politics per se. For instance, there is Garcia's *La Causa Politica* [1974], Garcia and Garza's *Chicano Political Experience* [1976], Rodolfo Acuna's *Occupied America* [1981], and Joan Moore and Harry Pachon's *Hispanics in the United States* [1985].

The stated premise of the work is that "Hispanic groups in this country have reached the point where they must accept being categorized as a single ethnic group" (p. 1). One could quibble with some of the terms Vigil uses. He effectively makes a case for the need for different Latino groups (Mexican Americans, Puerto Rican Americans, Cuban Americans, etc.), to "homogenize" under a common name in order to engender a political "oneness" such as has been prevalent among blacks but has not taken root among Latinos. However, the term of choice for Vigil—"Hispanic"—is one that inspires criticism and division in Latino communities, particularly among community activists who prefer the self-imposed term *Latino*. Many activists, primarily on the political Left, argue that *Hispanic* is akin to *Negro*, in being a term largely applied from the dominant culture and not one of the group's own choosing. In the same vein, Vigil frequently uses the terminology of the dominant culture—*illegal aliens*—when referring to immigrant Latino workers living in the United States without legal documentation. Latino activists, especially those working with such populations, prefer the less offensive term, *undocumented worker*. Finally, an avoidance of class and ideological themes is replete throughout this work. Dr. Vigil's focus on the necessity of creating broader in-group patterns among Latinos is well taken. However, more attention should be devoted to discussing class and ideological differences and cleavages that work against unity.

Vigil's first chapter begins effectively by recognizing erroneous implications of the dominant pluralist explanations of "democratic homogeneity." The dominant (pluralist) view is that we have a class-blind and a color-blind electorate in the United States. He effectively argues that on the contrary, U.S. history reflects racial-ethnic voting blocs, although there continues to be reluctance to recognize this since "it runs counter to certain propositions about the homogeneity of American culture and life." He then proceeds to inform us that "the first task that faces the social scientist who seeks to study one particular ethnic group . . . in American politics is to place that group in the context of traditional and modern conceptions of democratic political theory and the dynamics of American culture and intergroup relations" (p. 12).

However, the author falls into the self-imposed trap of theoretical myopia inasmuch as he strictly limits these conceptions to the two dominant theories of pluralism and elite theory before briefly considering alternative theories (internal colonialism, assimilation, melting pot, and cultural pluralism). His major omission here is that of Marxist class theory. He does not offer a critique from a Left political perspective, nor does he offer a discussion of class perspectives. However, the merit is that he explores the contradictions within pluralist theoretical homogeneity, a theme more extensively explored by Bayes in 1982.

Vigil's second chapter seeks to place the political juncture of today's Latinos in a mileau of historical understanding. As such it traces immigration patterns, levels of educational and occupational attainments, and the levels of socioeconomic accommodation of each. The third chapter, on the Hispanic vote, concentrates on the Latino vote in presidential elections, especially those in 1980 and 1984. It is valuable in that few scholars have attempted to assess the impact of Hispanic presidential votes and voting patterns, especially when compared with blacks. As informed as the chapter is, much of it seems veiled in excessive optimism—from his belief in the plausibility of biracial coalitions to his undue concentration on Hispanic electoral victories and minimizing of defeats. Nevertheless, there are high points like his discussions of political socialization variables and factors contributing to Hispanic political participation.

The real meat of the book, Chapters 4 and 5 (covering almost half the book), deals with Hispanic "success" stories in selected state and local elections in selected states. Herein there is both a positive and a negative. The positive is that it is extremely helpful to have listings and historical sketches of Hispanics who have achieved elective and appointive public office. The downside is that the focus is too narrow, too individualized, and too descriptive. There is little-to-no theorizing, modeling, or connection to voting rights legislation attempted. There is little to connect these individualized success stories with previous chapters. Vigil's sixth chapter is a useful if incomplete one on Hispanic organizations. His classification of organizational types makes little room for community-based and Left political-advocacy organizations.

Vigil sees a "new Hispanic politics" manifested by a "new style Hispanic politician," a new interest in registering Hispanics, and new Hispanic organizations that "have emerged to advance the interests of Hispanics through traditional lobbying strategies" (p. 134). While this is accurate, further discussion and elaboration among political scientists is needed.

JAMES A. REGALADO

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The Truly Disadvantaged. The Inner City, the Underclass, and Public Policy. By William Julius Wilson. Chicago: University of Chicago Press, 1987. 254p. $19.95.

Wilson argues that the problems of the worst-off inner-city blacks result from complex demographic and economic changes, not contemporary racial discrimination or the erosion of the work ethic. Together, the social transformation of the city and the reorganization of the economy have created this "underclass." The continuous flow of black migrants to urban centers has produced a large pool of relatively young, poor, and uneducated minority labor. At the same time deindustrialization, the growing proportion of knowledge-based jobs, and the movement of industry from urban to suburban locations have sharply reduced the number of entry-level, blue-collar positions that would allow these people to build and sustain intact families and work their way out of poverty. The exodus of the black working and middle classes from ghetto neighborhoods compounds the problem by undermining the social, religious, and neighborhood institutions that might integrate the most disadvantaged into stable, community networks. The results, Wilson suggests, are unprecendented "social pathologies" in the center cities, including skyrocketing rates of joblessness, teenage pregnancies, out-of-wedlock births, single-mother-headed families, welfare dependency, and street crime (p. 6).

A social democratic critic of the U.S. welfare state, Wilson rejects both liberal and conservative analyses of the Great Society and standard explanations of the problem of poverty in the United States. Building on arguments he advanced in *The Declining Significance of Race*, the author criticizes liberals for unduly emphasizing the role of racial discrimination in explaining black poverty. Current racism does not play a central part in the creation and reproduction of the underclass, he maintains. On the contrary, since the 1960s federal programs have eliminated many of the most egregious barriers to formal racial equality. Historical discrimination has helped, of course, to create a racial division of labor that now plays a powerful role in reproducing black inner-city poverty. But, Wilson insists, "broader issues of economic organization" that "have little or nothing to do with race" more powerfully explain the fate of the underclass (p. 12). Yet liberal reformers have failed to confront the class issue. Instead of committing the federal government to job creation, they recommend policies that target social benefits on a narrowly defined racial constituency.

Neoconservatives are dismissed altogether. Charles Murray and Lawrence Mead, the author suggests, continue to blame the victim by carelessly misapplying Oscar Lewis's culture-of-poverty argument without having fully understood it or checked the facts. More fundamentally, they appear oblivious to the analytic pitfalls inherent in using explanations of social behavior that give causal primacy to individual values over the situations and social structures in which people find themselves. This cultural view is taken seriously not because it makes sense but because liberals have failed to offer a coherent alternative.

Wilson's prescriptions flow directly from his diagnosis. Referencing Western European social democratic welfare states, he argues for an interventionist economic program that would keep labor markets tight and United States industry competitive by promoting economic growth and industrial restructuring. Means-tested and race-specific policies should be deemphasized in favor of universal entitlement programs that would simultaneously aid the most disadvantaged inner-city residents while benefiting the white majority. Only these can win the broad political support that is necessary if inner-city poverty is to be addressed successfully.

On balance, this is an important and impressive work. It correctly recognizes that race and class have combined in complex ways to create the underclass, and it makes a significant contribution to unraveling how. The comparative perspective, though not fully developed, is also welcome, as is the effort to situate the growing body of statistical work on urban poverty in an historical and sociopolitical context. There are problems, to be sure. The definition of the underclass—"the most disadvantaged segments of the black urban community" (p. 8), including the long-term unemployed, the unemployable, the poor, the welfare-dependent, and street criminals—is analytically diffuse, combining economic categories with forms of deviant behavior. The normative subtext is troubling: Wilson seems too willing to accept conventional notions of family structure and social pathology. It is not at all clear, for example, what black, single mothers in the ghetto have done wrong other than be poor. Finally, while the effort to focus attention on class is welcome, the claim that racial discrimination is no longer significant is probably an overstatement. Nonetheless, Wilson's mastery of the literature, careful scholarship, and imaginative proposals make this a compelling account of the crisis of the inner city and a powerful example of what deft hands can do with policy analysis.

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COMPARATIVE POLITICS

Outside Moscow: Power, Politics, and Budgetary Policy in the Soviet Republics. By Donna Bahry. New York: Columbia University Press, 1987. 236p. $30.00.

Ever since Merle Fainsod's *Smolensk under Soviet Rule* (1958) provided an early glimpse of life away from the center, there has been an increasing fascination with local (republic-level or below) politics or policy-making in the Soviet Union. The death of Stalin and demise of the totalitarian model other than as an "ideal type" have opened the door to numerous studies of the local scene, including Philip D. Stewart's *Political Power in the Soviet Union* (1968), Jerry Hough's *Soviet Prefects* (1969), William Taubman's *Governing Soviet Cities* (1973) and Ronald Hill's, *Soviet Political Elites* (1977).

However, less attention has been focused on the relationships between the center and the periphery. Donna Bahry attempts to fill the gap through an analysis of "politics and budgetary policies at two levels of government—union republic and local" (p. 11). Her focus is on both qualitative material (speeches of republic and local officials at CPSU party congresses and Supreme Soviet budget sessions) and quantitative data (budgetary and investment figures for Soviet republics, especially since the 1950s).

Bahry's initial chapter compares center-periphery relations in the USSR with those of several Western nations. While this seems like a fruitful topic, she returns to it only briefly at the end of the book. However, she has opened the door for what could be a promising area of comparative analysis. Succeeding chapters discuss the general development of Soviet "fiscal-federalism," consider the republics' role in the budgetary and planning process and the nature of the agenda put forth by republic officials, analyze the impact of several variables on policy outcomes, and examine problems of local leaders—those below the republic level. The concluding chapter assesses the implications of her findings "for the study of policy-making and of political centralization in the USSR" (p. 12).

Bahry notes the difficulties associated with gathering and interpreting Soviet data and her appendixes provide useful commentary on these matters. Nevertheless, her analysis of these data is persuasive and she presents a thoughtful, if somewhat surprising, picture of trends in Soviet budgetary policy-making and relations between the center and those outside Moscow.

Since Stalin's death local leaders have gained in political stature (as measured by representation in higher bodies) and now have more programs and funds to administer. However, this decentralization requires greater coordination of efforts and a larger voice in decision making, factors that the center has been unable or unwilling to provide. As a result, republic officials need to lobby for greater resources and autonomy and find themselves as "players in an elaborate bargaining process between center and periphery" (p. 63). Their major efforts involve trying to influence planning decisions through various channels of access: Gosplan, individual ministries, the Council of Ministers, the Central Committee, Supreme Soviet sessions, and meetings of the Party Congress.

Bahry's analysis of republic leaders' speeches provides a picture of their lobbying agenda. The primary concerns raised by these officials relate to agriculture and related areas and to the development of energy, metals, and other natural resources. Problems relating to economic and social infrastructure and construction equipment and materials also appear frequently (p. 81). This is to be expected, since these areas provide republics with their major sources of revenue or are among those for which they share administrative responsibility with central ministries—without the power to control events. The nature of these concerns has remained steady over time and does not seem to be affected by political variables. Bahry controls for such factors as representation in the Politburo, whether a republic leader is native or from another republic, and whether the leader has been newly appointed and thus is someone who might feel free to be more assertive. None of these variables affects the nature of demands raised by republic officials.

The success of these appeals is examined through an analysis of Soviet appropriations in the areas of capital investment, enlightenment, and health care. Two patterns emerge. First, investment allocations are generally stable, showing only incremental changes over time. Second, the same political variables noted above have little if any impact on the nature of allocations. This pattern raises serious questions about the importance of political connections in the budgetary process. Republics with direct access to Moscow (e.g., with Politburo representation) do not fare better than others. Instead, Bahry suggests that Soviet national leaders may be pursuing a policy that emphasizes equality in republic allocations in an effort to increase their overall level of support throughout the country. The result may be a series of compromises that aim to keep everyone happy but may also reflect the fact that "there is simply no single grand strategy for regional growth" (p. 121).

Yet surely membership in the Politburo or access to others in Moscow does matter. Decisions regarding foreign policy, domestic priorities, current economic or political reforms, cadres' selection, and so on are still made at the center. But the scope of Bahry's analysis is limited primarily to budget-related data and does not address these issues at any length. After moving through her careful analysis, one longs for a livelier narrative. Some readers may yearn for a discussion of party infighting—Gorbachev and Ligachev arguing over the disposition of Yeltsin or the future of Shcherbitskii.

But that is precisely the point. Much of Soviet politics and economic decision making lacks the high drama of Kremlin intrigue. It is rather a more mundane process of trying to allocate a limited amount of funds (confounded by a significant lack of data and complex problems of coordination) to what must seem like an infinitude of reasonable demands made on the center by those on the outside.

Bahry's worthwhile effort raises serious doubts about whether such a planning process can ever work in other than an incremental and uncoordinated fashion. Even *perestroika* may not be able to bring it into line.

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Ethnic Conflict: International Perspectives. Edited by Jerry Boucher, Dan Landis, and Karen Arnold Clark. Beverly Hills: Sage, 1987. 332p. $29.95 cloth, $5.50 paper.

A variety of areas in political science could benefit from a serious examination of the impact of ethnic identification on patterns of conflict and cooperation. A volume subtitled *International Perspectives* with a table of contents offering case studies ranging from Sinhala-Tamil conflict in Sri Lanka to mechanisms for managing and diffusing antagonism among the variety of groups in Hawaii would seem to offer much. The scholar with an interest in comparative politics might hope to find an approach to ethnic conflict that identifies features common to several geographic and cultural regions. The student of international politics might approach the book looking for clues about the conditions under which internal ethnic problems lead to external intervention.

Those expectations are likely to be disappointed. The papers were originally delivered at a conference sponsored by the University of Hawaii's East-West Center in March 1985. The editors report that the conference participants sought to derive a list of themes that might generalize across all the conflicts discussed and might be used, at least descriptively, as organizing principles for the revisions leading to the present volume. Those themes, enumerated in an opening chapter by Dan Landis and Jerry Boucher, include perceived differences between groups, stereotypes, and ethnic identity; land tenure and homeland issues and immigrant-versus-native status; involvement of outsiders; disparate allocation of power and resources; effects of language and language policy; process of conflict resolution; and religion. For the most part the papers do not refer to the common themes, let alone use or share concepts or theoretical approaches. The final chapter, "Some Thoughts on Ethnic Conflict" by Winthrop D. Jordan, was intended as a summary. One can only sympathize with his proviso that the papers do not lend themselves to a coherent overview.

The central concept, *ethnicity*, is nowhere rigorously defined or clearly contrasted with other forms of politically relevant identity. For example, only a very diffuse and fuzzy definition of *ethnicity* can justify inclusion of the Malay Peninsula case (Ronald Provencher's "Interethnic Conflict in the Malay Peninsula," focusing on three groups—Chinese, Malay, and Thai—who spring from different ancestry, have distinctive cultural and linguistic patterns, and different historical relationships) and Rolf Kuschel's study of vengeance and feuding among the approximately five hundred inhabitants of one of the Solomon islands, whose patrilineal clans share descent from ancestors who arrived together as migrants, feature extensive intermarriage, and have a common language and culture.

Some of the cases discussed in the papers ("Sinhala-Tamil Relations in Modern Sri Lanka" by Sinnapah Arasaratnam, "The Basque Conflict" by J. Martin Ramirez and Bobbie Sullivan, Ronald Bailey's "Black-White Relations in Mississippi," Joseph E. Trimble's "American Indians and Interethnic Conflict," or Eric S. Casino's study of the Moros in "Interethnic Conflict in the Philippine Archipelago") have been marked by varying degrees of violence; others have not. But neither the chapters themselves, the editor's introduction, nor a foreword by Ross Stagner offer any systematic guidance in identifying the variables that might explain the presence of violence or its intensity.

Provencher's study of the Malay Peninsula, "Ethnic Conflict Management in Yunnan, China" by Zhang Shifu and David Y. H. Wu, M. Jocelyn Armstrong's "Interethnic Conflict in New Zealand," and John Kirkpatrick's analysis of Hawaii focus on situations that might well be regarded as examples of successful and basically cooperative relationships. Armstrong is quick to point out that the official myth of "one people" including the original Maori inhabitants and the Pakeha (white) immigrants has never been completely accurate and describes the rise of a Maori consciousness that has led to some low-key demonstrations and official attempts to institute reforms to reinforce biculturalism. She leaves the probability of success an open question, as does Kirkpatrick in summarizing the ability of Hawaiians to maintain consensual controls on potential stresses. But since the four studies are descriptive and rely heavily on idiosyncratic features of the local scene to account for outcomes, the relevance of the cases to other multiethnic settings is not clear.

It is difficult to see many ways in which a political scientist might put this work to use. A reader with little or no prior knowledge may, as I did, find several of the pieces interesting introductions to situations that do not receive a great deal of publicity but clearly matter to those involved. Area specialists are unlikely to find anything particularly new or innovative in what are, of necessity, quick overviews. Although some of the conflicts have had an important international dimension there is little analysis of the dynamics or impact of escalation or the role of external actors. This book is also unlikely to serve as either a text or set of readings for a course dealing with ethnicity and politics, given its lack of coherence and framework and scattershot geographic coverage.

SETH THOMPSON

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Sex Differences in Political Participation: Processes of Change in Fourteen Nations. By Carol A. Christy. New York: Praeger, 1987. 195p. $37.95.

In 1978 Sidney Verba and his colleagues reported a persistent pattern of sex differences in political activity in the seven nations included in their survey of political participation (Verba, Nie, and Kim, *Participation and Political Equality,* [1978]). In this volume, Carol Christy proposes to examine, first, if these differences have diminished over time, and second, whether the generation, diffusion, or development models adequately explain those instances where reduction has occurred.

Her first problem is assembling reliable data. As she notes, trend data are needed for testing the three models of change. However, trend data were available only for the United States and West Germany. For the most part, therefore, her analysis is based on cross-national comparisons. She examines data from 27 surveys conducted between 1958 and 1980 in 14 countries. Christy recognizes the shortcomings of this procedure, noting that "while nontrend data permit the examination of more diverse types of nations and political participation, they also require making certain nonverifiable assumptions about the processes of change over time" (p. 14). She infers changes over time from data that measure differences across space. On the basis of her analysis, Christy concludes that while all three models are useful for explaining variations in sex differences, short-term forces operating outside these models also play an important role: "No single factor explains trends in sex differences in political participation" (p. 102). She notes, for example, the influence of feminist activity in West Germany and the impact of Reagan's stand on women's rights in the United States on women's political participation.

The question of sex differences in participation, or what in some contexts might be called the gender gap, is one that has become more important in the political behavior literature during the past 15 years. Much of the growing body of literature in this field stresses the importance of traditional socialization patterns that have channeled men into public or political activities and women into private or household activities. Increasingly, researchers have come to recognize as well that structural or situational obstacles such as party gatekeepers or the demands of a double day help to sustain sex differences in participation. Unfortunately, this study does not permit examination of the impact of these factors, since it only measures political participation as a function of socioeconomic indicators. Like many comparative analyses of political behavior, this investigation of sex differences suffers from both too much and too little data. The task of sorting through 27 surveys spanning over 20 years is a difficult one, even when a limited number of independent variables is used. The result, in this case, is a complex analysis that yields relatively little useful information. In the end Christy resorts to "human choice and action" over "abstract, mechanical forces" in her search for explanatory variables (p. 115). Her quick trip through 14 countries yields little new or substantive information about either women's or men's political participation in these countries.

Chapter 4 stands out as an exception. In this chapter on differences over time, Christy is forced to restrict her analysis to the two countries for which she has trend data, West Germany and the United States. Here we get a good test of both the development and the generational models. The author concludes that both models are only weakly supported. But the generational model has been applied successfully in studies of women's political participation in France and in Canada. If this study is to improve our understanding of cross-national differences, the apparent conflict between Christy's findings and those of other researchers needs to be addressed.

This failure to situate the study in the context of our understanding of women's political participation reflects a major weakness of the book. The comparative literature in this area has grown dramatically since the early 1970s, when the first attempts to include women in analyses of political behavior were made. One of the most positive contributions made by feminist scholars in political science has been the emphasis on qualitative research as a necessary complement to quantitative research, and on the sometimes different meanings assigned to politics and to political activity by women and men. In this study there is an absence of qualitative data. In addition, there has been little attempt to assess the impact of political culture, changes in the legal status of women, or the ideology of the party in power on women's ability and willingness to take part in the political process. Nor has the author demonstrated her sensitivity to the different forms of participation favored by women (in particular, group activity and personalized contacting).

As a result, Christy is able to demonstrate conclusively that a gender gap in some forms of political participation and related attitudes has narrowed over time but still exists in a wide range of societies. She is not able to explain why this sex difference persists. In the end this book is more valuable as a critique of existing data and data collection techniques than as a comprehensive analysis of sex differences in participation.

SANDRA BURT

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Public Interest Law. Edited by Jeremy Cooper and Rajeev Dhavan. Oxford: Basil Blackwell, 1986. 482p. $75.00.

This volume is particularly welcome because it provides a comprehensive and challenging view of the hitherto neglected activities of British public interest lawyers—thus setting the scene for comparative research on problems that have for too long been dominated by the U.S. perspectives. While there are some books and articles on various aspects of British public interest law, there is nothing else that approaches the scope and coherence of this collection. The eclectic group of (mostly British) academics and activists has managed to provide both a lively debate and an effective primer for newcomers to the topic. The general quality is high and the essays hang together surprisingly well.

The scope of the enterprise is clearly conveyed by Cooper and Dahvan's definitional point of departure: "Public interest law consists in the use of litigation and public advocacy (i.e., lobbying by representation or publication) to advance the cause of minority or disadvantaged groups, and individuals, or the public interest" (p. 5). The central question posed by the editors is what role public interest law and lawyers can play in alleviating "the deepening concern about increasing social and economic differentials between various classes, economic recession and political tension" (p. 3). The contributors are in sharp and stimulating disagreement about how this question is to be answered.

But before getting into that debate, consider the basics. There are, in essence, three components to British public interest law: private practitioners funded primarily by legal aid; salaried lawyers working in community-based law centers; and social action groups organized to protect women, consumers, the disabled, immigrants, the mentally ill, civil liberties, and so on. While each of these components is given some attention, the major focus is on the social action groups, which, as Dhavan sees it, have been the most enterprising and savvy: "Social action groups in England have demonstrated, in abundant measure, their capacity to be involved in sophisticated legal campaigns in a multiplicity of legal, social and political arenas" (p. 37). This inventiveness seems particularly important in the United Kingdom where, as Carol Harlow points out, the courts, "though increasingly politicized . . . have not emerged as protectors of the poor. Nor have they generally provided a platform for the expression of minority views" (p. 104). Moreover, because there is no written constitution or bill of rights, occasional judicial victories can frequently be neutralized by the government's parliamentary majority. In sum, as a primer this volume offers both an introductory overview of the many facets of British public interest law and an analysis of the constraints imposed on public interest lawyering by the restrictive orthodoxy of the British legal culture.

But the broader message has to do with the relevance of public interest law to transformative and reformative politics in contemporary Britain. On this matter, the authors are sharply divided, with essays by Dhavan and Richard Abel framing the debate. Dhavan's two essays (one written with Martin Partington) challenge public interest lawyers to adopt and pursue a broad agenda of welfare, justice, and civil and political rights aimed at empowering the disadvantaged and at reversing the top-down flow of welfare state politics. Abel forcefully rejects this strategy: "We must not assume that because law is an essential shield against the abuses of state power it can be wielded as a sword to alter fundamental class relations" (p. 383). While Abel and Dhavan would agree that legal victories are hard to secure and of limited immediate benefit to disadvantaged interests, they differ on the long-term potential of a politics of rights.

To Abel, the politics of rights is an illusion—dependent on state-financed legal aid made available in a spirit of noblesse oblige: "Legal aid is an attempt by those who enjoy state power, ownership of capital, and patriarchal domination to convince themselves that those privileges are not abused through arbitrary action, exploitation, violence, or irresponsibility, and that any abuses are promptly redressed" (p. 389). Dhavan, in contrast, believes that public interest law has the potential for redefining the terms of political discourse and for contributing to political mobilization. For Dhavan, the problem is not the inherent limitations of a politics of rights but the failure of social action groups to seize the opportunities provided by public interest law.

With Abel and Dhavan framing the debate

and the other essays providing an introduction to the mechanics of judicial and administrative activism as well as some case study "raw material," this volume enables readers to begin to draw their own, albeit preliminary, judgments about the political relevance of British public interest law. The book should, therefore, be useful to students of British and comparative politics as well as to specialists in public law.

STUART A. SCHEINGOLD

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Apartheid's Rebels: Inside South Africa's Hidden War. By Stephen M. Davis. New Haven: Yale University Press, 1987. 272p. $27.50 cloth, $8.95 paper.

The Politics of Economic Power in Southern Africa. By Ronald T. Libby. Princeton: Princeton University Press, 1987. 361p. $40.00 cloth, $12.50 paper.

These two books on South Africa present remarkable contrasts. Both tackle important subjects. Davis's *Apartheid Rebels* is a fluent presentation of the aims, organizations, and capabilities of the contemporary African National Congress (ANC). Libby's *Politics of Economic Power* examines the regional economy of southern Africa from the periphery as well as the center and shows how political considerations influence economic decision making. The first book is journalistic and only superficially analytical. The second embeds case studies in theory; proposes new contributions to the study of the state, of dependency, and of political economy; and takes itself very seriously. Yet the first is the more important of the two.

Davis cannot claim to have understood precisely how the ANC operates either militarily or politically from its bases in Angola or its political headquarters in Zambia. Nor can he do more than describe infiltration routes into South Africa and enumerate some of the tactical considerations of a "humanitarian" guerilla struggle prosecuted by vastly outnumbered and outgunned black cadres. There are others who have a fuller appreciation of the ANC's nuanced ideological stance and its overall strategic program. Nevertheless, Davis adds an immediacy and an informed observer's gloss to existing appreciations of the ANC as an exiled entity capable of harassing Africa's most powerful state.

Libby accepts the realities of that power. South Africa dominates the southern African region (Libby carries the region north to Zaire and Tanzania but perversely excludes Angola). Nevertheless, he persists in casting the framework of his book within the shadow of South African hegemony while simultaneously insisting upon the analytical importance of what truly is a very simple and hardly arguable point: that the policies of the hegemon are also influenced by the economies of the separate states of the region. He posits interdependence in place of dependence, but no local observer could argue otherwise. It is the scale that is important and the ease with which South Africa uses its economic leverage (and overwhelming leverage) to destabilize even the least ravaged of the local economies.

Libby describes the interplay of economic activity throughout the region. Its rail routes, harbors, imports and exports, patterns of labor migration, and flows of deferred wages and remittances are set out well, though the data are no longer current. Indeed, Libby's approach depends as much as Davis's on being timely. Using dated, and thus suspect, material on trade, Libby contrives arguments already falsified by changing circumstances and altered policies. He also seems unaware of the more thorough and more supple discussions of many of the same problems by Gavin Maasdorp and Henry Bienen.

Libby's case studies are meant to exemplify his central theoretical reconsiderations. But they are dependent upon empirical research or at least empirical observations and betray confusion more than complete understandings rooted in nuanced knowledge. For example, the South African corporate community and multinationals with bases in the region would be startled to read that the ruling National party had *aligned* itself with them in order to cope with external and internal threats to the state. Libby tells us that the state sought to use transnational entities to divide Africans and to increase its leverage against hostile neighboring nations. He further asserts that the state's policy of constellationism had been intended to expand South Africa's market for manufactured goods and to mitigate the hostility of neighbors like Zimbabwe.

A few interviews (Davis's main form of research) with capitalists or officials might have modified Libby's assumptions about the alignment between the corporate world and the state in South Africa. Industry and foreign-owned multinationals have indeed been used for repressive and cooptive purposes by South Africa, but hardly is it or was it an alignment, and the manipulated employment of nonstate enterprise beyond South Africa's borders has been rare and hardly successful. Today, too, the rupture between corporate endeavor and the state is almost complete. Although enterprises do comply with officially imposed requirements and therefore assist state-sponsored repression and control, they also oppose the state, and the largest of them have systematically rejected anything that could be called an alignment.

This is but one disagreement. Libby makes his case most often with structural arguments rather than examples. He misconstrues the relations between industry and labor, neglects the historical dimension of regional manufacturing and the location of petroleum refining, takes housing issues out of context, and implies that the National party actually sought reform (linked with big business) before its well-intended policies foundered on the rock of Conservative party opposition.

The other case studies of the smaller economies of the region are presented with varying degrees of success. The description of Zimbabwe's situation is limited to a now-overtaken conflict between the political pulls of populism and the incentive-based technocratic imperative. Libby focuses on the manufacturing sector when the driving engine of Zimbabwe has been and will continue to be agricultural performance.

Among the best summaries of the interplay of politics and economy is the exposure (admittedly based on the research of others) of President H. Kamuzu Banda's authoritarian personal design in Malawi. His Press Holdings Ltd. dominates the country for personal as well as political gain. The discussion of Mozambique is also good, if only of historical interest now that the attacks of Renamo, a South African proxy force, drought, and mismanagement have forced the poor state to its knees. The main issue, unaddressed by Libby, is why South Africa refuses to take advantage of its new patron-client relationship with Mozambique and persists in destabilization instead of development.

Libby misses the main economic question about Zambia—how to restore incentives to a very heavily urbanized polity, wholly dependent upon a single commodity, where leadership is ignorant. His chapter on Namibia likewise focuses on future land issues when the someday independent state's prominent feature will be its continued structural reliance on an overweening neighboring economy as the real value of its copper, uranium, and diamonds diminishes.

Davis's book can be faulted for its failure to probe skillfully below the surface of rhetoric or received information. Libby's overarching structure is flimsy and simple, and his case studies are either misleading or summary. Even so, his conclusion does embody one significant, if banal, policy truth: that the smaller economies of southern Africa are both dependent upon South Africa and at the same time capable of exerting a tug of their own on that powerful polity.

ROBERT I. ROTBERG

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Intellectuals in Liberal Democracies: Political Influence and Social Involvement. Edited by Alain G. Gagnon. New York: Praeger, 1987. 242p. $39.95.

Intellectuals love to generalize. Yet very few generalizations about the current relationship between intellectuals and political systems command agreement. That may be because intellectuals also love to disagree and to coin ever-differing interpretations of their own role in politics. But controversy over changes in that role may also be strongly influenced by locale. It is possible, for example, that in some societies intellectuals may continue to be marginalized while in others they are increasingly admitted or recruited into the circles of power. I speak not only of Jewish intellectuals and Marxist intellectuals but also, with distinctive connotations, of English, French, and third-world intellectuals. It is a virtue of this book that its strongest chapters blend questions of perennial concern to the sociologists of intellectuals—whether intellectuals form a separate class, whether social marginality account for criticism or alienation, and so on—with a nearly equal concern with the impact of the differing national political contexts in which intellectuals are rooted.

In my view the principal contribution of the collection comes in seven case studies of the role of the intellectual in the "liberal democracies" announced in its subtitle: Canada, the United States, England, France, West Germany, Italy, and Israel. Supplementing them is a rather miscellaneous section devoted to "perspectives for comparative analysis," which contains familiar total systems comparisons by S. N. Eisenstadt. A provocative wrap-up chapter by Robert J. Brym takes a hard look at the main sociological interpretations of intellectuals in political life, dismissing social rootlessness, malintegration into society, and "new class" expertise as explanations for newer roles of intellectuals in favor or a Gramscian interpretation based on social origins and the structure of career opportunities.

The study of intellectuals is an area of inquiry especially lacking in agreed parameters or methodolgies, and this collection, for all its numerous good parts, demonstrates rather than resolves that condition. The contributors seem to agree only that the status of intellectuals in liberal democracies is in flux and needs explaining. They differ in varying degree on the nature of such change and on general theory that might explain it. That does not mean that insightful middle-range propositions, derived from individual national experiences, are absent. For example, in a persuasive analysis of why Left intellectuals seem to have disappeared in France today, George Ross dwells on the effects of "the changing professional market" for Left ideas in a society increasingly dominated by mass rather than elite culture. Carl Boggs' examination of Italy concludes that while "free-floating" intellectuals may have disappeared, their "modernizing" cousins flourish inside the semicorporatized world of party bureaucracies, public corporations, and interest groups. This decline of the marginal (usually Left) intellectual is echoed in William Rubinstein's lively depiction of the transformed situation of Jewish intellectuals in a number of national settings. On the other hand, Paul Hollander's rather polemical essay on the United States finds the marginalized social critic alive, well, and destructive in the United States, a position that can be read either as a confirmation of U.S. exceptionalism or simply as reflecting a blinkered perspective.

Fifteen years ago Charles Kadushin (quoted p. 208) wrote on the study of intellectuals that "theory building in this field has been marred by an abundance of opinion and moralization, a dearth of facts, and a plethora of parochial definitions." By itself the Gagnon volume does not overturn that judgment; but, especially in its presentation of comparative perspectives, it softens it a bit and thus is welcome.

JAMES B. CHRISTOPH

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The Military in African Politics. Edited by John W. Harbeson. New York: Praeger, 1987. 208p. $35.95.

The task that Harbeson sets for himself and his colleagues is to challenge the dominant assumptions of much of the existing literature on the African military and to establish the foundations for a second generation of studies of military rule in Africa. Harbeson asserts that military intervention in civilian politics in Africa is not a "temporary aberration" in the political development of the continent but must be treated as "part and parcel" of that process (p. 2).

Rather than focusing upon how and why the military intervenes, two sets of questions form the central theme of this volume: (1) How do military officers behave in power? How do they mobilize resources in order to legitimize their assumption of power, and what policies do they pursue in order to address problems such as economic stagnation and ethnic strife, which led them to intervene in the first place? and (2) How do military rulers use their power to change the written and unwritten rules that regulate both public authority and political activity in Africa? What role do military rules play in the process of transferring political power from one leader to a successor?

While these questions are interesting, Harbeson relates them to the growing debate on the nature of the state in Africa. If the military is in fact equal rather than subordinate to civil authorities, then, Harbeson argues, theories of the state in Africa must transcend both Marxist and liberal democratic theories, neither of which emphasizes this equality (p. 5). While this theoretical dimension is critical to the study of the African military, it is not fully developed in this collection.

Analyzing the role of the Sudanese military, Nelson Kasfir examines how former President Nimieri's strategy of managing support for his regime by constantly courting new allies and turning his back on old ones, eventually cost him the support of his one essential backer, the armed forces. In contrast, Dov Ronen argues that the current military regime in Benin has been able to expand its support significantly. Ronen argues that much of this success can be attributed to the ability of the military to redefine salient social cleavages away from patterns of ethnic identity and—with the introduction of Marxist-Leninist rhetoric—towards ideological affiliation. Military management of ethnic cleavage also forms a central theme in Victor LeVine's essay on the Congo.

Two essays focus on attempts by military rulers to engage in political and economic transformation. Henry Bienen argues that West African "populist" military regimes in Liberia, Ghana, and Upper Volta have sought to reestablish ties between the governing elite and the broader population by creating new political institutions. While bypassing traditional structures and violating established rules of political behavior, Bienen concludes that attempts by the military to establish new popular power bases for their regimes have been much more successful than similar attempts at economic transformation. Such a conclusion stands in sharp contrast to that of John Harbeson, who argues that under the current military regime in Ethiopia, the political relationship between the centralized state and the peasantry bears a striking similarity to that which existed under the former imperial regime. The Ethiopian Derg has, however, been far more successful in engaging in economic transformation, particularly in the areas of literacy and land redistribution.

I. W. Zartman focuses his analysis of Algeria upon two contrasting patterns of military behavior. During periods of political stability, policy-making is characterized by patron-client relationships that enable various groups within the military and civilian sectors to form alliances. During periods of political succession, however, the military displays a much more cohesive pattern of behavior. Similarly, in his study of Nigeria, Richard Joseph examines how the military has not only sought to restructure the polity but has also had a "profound impact on the rules that determine the legitimate exercise of public power" (p. 69).

The primary strength of this volume is the way in which it reconceptualizes the role of the military in Africa as being equal to that of the civilian authorities. With the exception of Algeria all of the countries examined in this volume have experienced a dramatic and overt assumption of political power by the military. An alternative road to parity might be much more incremental. In cases where the survival of the dominant social structures becomes increasingly dependent upon the use of force, the military might well see its role increase beyond parity, without the need to displace formal civilian political leaders. The obvious case in point is that of South Africa, where maintenance of the white regime has resulted in the civilian restructuring of formal political institutions in such a way that power has flowed rapidly into the hands of military leaders and their close associates.

The greatest weakness of this volume is that it excludes from its list of case studies southern Africa, the very region in which military-civilian collaboration has its deepest historical roots. The imposition and maintenance of settler colonial regimes throughout this region necessitated close cooperation between civil and military authorities throughout the entire colonial era. Similarly, one of the major political legacies of the struggle for independence throughout the region has been the need to unify the military and political structures of the various liberation movements. While the research task of this volume is solid, its findings could have been enriched significantly by examining civil-military relations both in settler state structures and African liberation movements, as well as in postindependence regimes.

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Book Reviews: Comparative Politics

Thatcherism and British Politics: The End of Consensus? By Dennis Kavanagh. New York: Oxford University Press, 1987. 334p. $44.00 cloth, $11.95 paper.

When Prime Minister Margaret Thatcher led her Conservative party to its third straight general election victory in June 1987, she paved the way to surpass all other twentieth-century British prime ministers in length of continuous service in that office. On 3 January 1988 she achieved that record. Under her leadership many changes in British politics, economy, and society in general have resulted from the impact of what has come to be labeled "Thatcherism."

The title of Dennis Kavanagh's new book, *Thatcherism and British Politics*, points up something new and important as a force in British political life. No previous prime minister's name has been turned into an *ism*. Kavanagh undertakes a thoughtful and thorough analysis of what this development means to contemporary Britain by examining the "consensus politics" he sees preceding it, the forces that destroyed that consensus, and the prospects for the future.

In postwar Britain a political consensus developed, based on broad agreement between the major political parties' front benches on a mixed economy and welfare state. The author traces the challenges that undermined consensus and led to its breakdown in the 1970s, assessing the role of the major actors and groups involved. He then evaluates the development of a British "New Right" agenda in the 1980s. The New Right policies provide the doctrinal substance that, combined with the prime minister's unique personal style, constitutes Thatcherism.

Numerous books and articles on British politics and Thatcher leadership are currently appearing. Margaret Thatcher has become something of a "hot topic" among political analysts as she moves into seniority both domestically and on the international scene. The British political system is also more in vogue as a focus of study than it has been recently because of the changes in what many considered its static and relatively unproductive consensus politics. Most current studies have been either leader-personality-oriented or analyses of the policy-institution-process interplay. A particular merit of Kavanagh's book is its more ambitious attempt to combine analyses of events, policies, processes, and personalities. In this he is largely successful, particularly with his treatment of Thatcher's style of leadership.

One of the most valuable chapters in this book is "Margaret Thatcher: A Mobilizing Prime Minister." Here the author applies the distinction between mobilizing and reconciling leadership styles to Thatcher, concluding that she is a "mobilizer in a rather immobilist society" (p. 278). As such, he finds her to be an unusual Conservative: activist and confrontational, forceful, radical, and practical in approach. In a system in which the prime minister is supposedly constrained by forces that hinder a U.S. president less or not at all, Thatcher captured her own party by surprise and has been able to exert a great measure of personal influence. The author evaluates the prime minister as party leader, cabinet manager, parliamentary force, and public figure. In contrast to other Conservative leaders, who traditionally work for compromise and accommodate to major interests (a style especially suited to consensus politics), he finds Thatcher's assertiveness and her determination to head a "conviction government" (p. 253) have resulted in both strong leadership and polarization. There is little middle ground. She is loved or hated.

The most unfortunate feature of this otherwise valuable study is the timing of its publication. Appearing in May 1987, it just missed the 11 June general election. This does not reduce its worth as an analysis of new directions in British politics through 1985–86, but it does leave the reader with some frustration since the campaign and election intervene between the writing and reading of the book. It leads to such then-reasonable observations as "By 1986 the tide of opinion seemed to have turned against the government. There seems little doubt, however, that she has left her mark on Britain in the 1980s and shaped the politics of the post-Thatcher era" (p. 317). It is certainly true, but in the wake of the 1987 general election outcome, this post-Thatcher era is some time distant. In light of Kavanagh's previous British election studies, the reader will regret the absence of coverage of this major election from his book, especially some further evaluation of the possible "new consensus."

Kavanagh provides a useful, informative, and readable—if already somewhat dated—book that forecasts a new consensus for Britain, a possible middle ground between the old Labor positions and those of the New Right. This is likely to include acceptance of much of the welfare policies from the past and new Thatcher economics, Kavanagh speculates. Whether or not this combination can actually produce consensus remains to be seen, but it is made more problematic by the North-South polarization of Great Britain as revealed in the 1987 election outcome.

BARBARA B. KNIGHT

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The French Socialists in Power, 1981-1986. Edited by Patrick McCarthy. Westport, CT: Greenwood, 1987. 212p. $39.95.

The Socialist government that ruled France between 1981 and 1986 was unprecedented: it marked the first time in the Fifth Republic that the Left was elected to government between opposing political coalitions and the first time in the twentieth century that the French Left remained in office for much over a year. Although the Socialist party (PS) lost the 1986 parliamentary elections, it remains France's largest political party and its future could not be more rosy. A key development since 1982 is that the PS flourished while thoroughly renouncing the leftist orientation that it espoused throughout the previous decade. Moreover, it is ironic that during the PS's tenure there was a marked decline in the degree of class confrontation and statism, which have traditionally characterized French political culture.

The French Socialists in Power, 1981-1986 is a fine description of the fascinating period by leading U.S. and British scholars. It compares favorably with other recent treatments, including volumes edited by John Ambler, Philip Cerny and Martin Schain and George Ross, Stanley Hoffmann, and Sylvia Malzacher. Although much will be familiar to specialists, several essays make original contributions. Michael M. Harrison illuminates the complexities of French foreign and security policy, focusing on the ambivalent relationship between Mitterrand's France and Reagan's United States. On the key issue of East-West relations, Mitterrand vociferously supported siting U.S. missiles in West Europe—although of course not on French soil—but denounced Reagan's attempt to prevent European nations from cooperating with the Soviet Union in constructing the gas pipeline. Harrison praises the government for having "managed the difficult feat of reconciling a form of restrained Atlanticism with the Gaullist heritage of national independence and European aspiration" (p. 59).

Michalina Vaughan usefully clarifies the Socialist government's complex, bewildering, and controversial educational reform projects, one of which brought down the first prime minister. Vivien A. Schmidt rightly emphasizes the importance of the decentralization reform. However, she exaggerates the impact of the Socialist-sponsored restriction on multiple officeholding (*cumul des mandats*): few elected officials exceeded the liberal limits established by the reform.

George Ross provides a characteristically stimulating account of the troubled relationship between trade unions and the Socialist government. Given the weakness, fragmentation, and ideological divergences within organized labor, it was impossible for the government to satisfy unions. Moreover, he asserts, in an era of economic crisis and union decline the Socialist reform of industrial relations (the Auroux laws)—which (in his view) sought to strengthen unions—actually benefited business. However, did the government in fact accord such high priority to strengthening unions? The Auroux laws had a multiplicity of goals, and certain provisions short-circuited unions.

Martin A. Schain contributes a superb analysis of the National Front's rise to respectability. He cogently suggests that the conversion of extensive antiimmigrant racist sentiment to support for the National Front was facilitated by the fact that leaders of every major party (including the Communist and Socialist parties) adopted antiimmigrant positions.

There are also useful contributions by Volkmar Lauber on the Socialists' Achilles heel, economic policy, Patrick McCarthy on the Socialist party's evolution in office; and D. S. Bell and Byron Criddle on the decline of the French Communist party (the PCF), which they rightly term "the most important development in French politics in the 1980s" (p. 156).

Several divergences occur within the volume—not a shortcoming, although the contending positions might have been developed more fully. A notable instance is differing assessments of the relationship between the Communist-leaning trade union (the CGT) and the government. Bell and Criddle assert that from the outset, the PCF used the CGT to express its distaste for the Socialist government. Ross sees the CGT (more accurately in my view) as "perhaps the *major* 'interest group' backer of the left between 1981 and 1984 (when the Communists left the government)" (p. 112). More significant than the CGT's intermittent criticism of government policies is that the union generally discouraged workers from mobilizing. (Indeed, the CGT's stance proved costly in its competition with other unions.)

A small disagreement and a large disappointment: Patrick McCarthy argues that the PCF "leadership offered Mitterrand its unconditional support" (p. 9) at the runoff ballot of the 1981 election. However, there is extensive evidence (some of which, admittedly, appeared after the book was published) that party leaders played a double game by publicly supporting Mitterrand yet privately seeking his defeat.

The volume illuminates important aspects of French policy and politics. Yet those seeking to understand the fundamental transformation in France's political, ideological, and economic landscape in the 1980s, as well as the Socialist government's responsibility for these developments, will be disappointed. In a nation long considered the epitome of class confrontation and statism, class identities and conflict have declined, new forms of cross-class compromise have developed, and support has increased for nonstatist methods of regulating conflicts. The Socialist government sponsored some of these changes, resisted others, and (by its failures) provoked others yet. *The French Socialists in Power* is a fine study of the trees. The forest, however, remains obscure.

MARK KESSELMAN

Columbia University

Politics and Sexual Equality: The Comparative Position of Women in Western Democracies. By Pippa Norris. Boulder: Rienner, 1987. 165p. $25.00.

The Women's Movements of the United States and Western Europe: Consciousness, Political Opportunity, and Public Policy. Edited by Mary Fainsod Katzenstein and Carol McClarg Mueller. Philadelphia: Temple University Press, 1987. 321p. $34.95.

Women's Rights in France. By Dorothy McBride Stetson. Westport, CT: Greenwood, 1987. $32.95.

In the decade since the first broadsides against the neglect of women's politics were fired, much has been done to redress the balance. The late 1970s witnessed the first blossoming of gender studies of U.S. politics, and in the 1980s comparative work has flourished. The three volumes under review, along with the recently published second edition of Vicky Randall's *Women and Politics* (1987), are part of this ongoing wave of research.

The three books are very different in conception. The prolific Pippa Norris, senior lecturer in government at Newcastle Polytechnic in Britain, has written a quantitative, comparative, multiple indicator study of factors affecting women's social, economic, and political equality in 24 first-world countries. The 14 essays in Katzenstein and Mueller are empirical—although seldom quantitative—studies of women's movements in seven Western countries. Coverage is uneven. The United States is the sole empirical referent of 4 chapters and is treated comparatively in 4 more, and Italy is the subject of 3. Stetson's book is a heavily documented examination of how, over the last 20 years, France has moved from being one of the Western European states least favorable to women's equality to a position of leadership in women's legal rights.

What unites these diverse approaches is a common emphasis on feminist movements as stimulating collective consciousness, institutional alliances, and public policy changes over the last two decades. All three volumes acknowledge that various strands of feminism, labeled radical (separatist), socialist, and liberal (reformist) exist; but the authors nevertheless see these groups as sufficiently united in the short term to have an impact. Stetson argues that feminist advocacy groups and their capacity to penetrate the state via ministries for women in the Giscard and Miterrand presidencies have been major agents of change in France. The Katzenstein and Mueller contributors are more concerned with how feminist groups have raised the general level of consciousness of women's political problems and how such groups have formed alliances, however temporary, with political parties. Public policy shifts attributable to these movements and alliances are not systematically analyzed across the seven countries. Norris, on the other hand, is the least concerned with the dynamics of group formation and consciousness raising but concentrates on the substantial public policy variations across Western democracies.

All three books explicate the problematic relationships of women's movements with leftist parties. As ideological advocates of greater social, economic, and political equality, such parties are often expected to be the natural allies of women's movements. But several analyses show that despite some notable successes, the relationships have been stormy. The key problem is that leftist parties continue to focus primarily upon the politics of class, which most feminists consider of secondary importance at best. Little serious attention is paid to how rightist groups and parties resist or accommodate feminist demands, a topic brilliantly analyzed by Jane Mansbridge in *Why We Lost the ERA* (1986).

This raises the issue of what theoretical contributions these volumes make. Norris casts her work in the familiar theoretical mold of the argument about whether public policy variations are due to socioeconomic forces only or are also influenced by political factors. She argues for the latter, claiming that leftist party control of government makes a difference for women's equality. This argument, however, does not hold for all areas of policy and is not buttressed by a detailed examination of leftist influence in any specific countries. Several of the Katzenstein and Mueller contributors utilize social movement theory, largely in an ad hoc fashion. Stetson concentrates on how feminists in France have been able to change the definitions of public policy issues affecting women over the years. She calls this approach studying the logic of policy problems. Aside from Norris, these perspectives rarely go beyond lower-middle-range theory.

What alternatives are there? An unresolved problem in these analyses is how to account for political change in the form of the rise and public policy impact of women's movements. Ronald Inglehart has argued that postindustrial society generates different value cleavages than industrial society. Despite much discussion of the feminist problem of allying with class-oriented parties, none of the authors in these books takes up the task of evaluating the utility of Inglehart's argument. Other possibly fruitful theoretical perspectives to use are those of political agenda, policy sector, policy culture, and corporatism. Some of these perspectives are given a passing nod, but nowhere in these books is there an extended application of any of them.

In short, what is needed now is a systematic, theoretically oriented, comparative attempt at explaining the growth and impact of women's movements across developed polities. Norris points in the right direction, but there should be more work sensitive to variations within countries as well as across them. Then one might be able to explain the Swedish paradox, in which governmental policies favorable to women's equality (many would argue *the most favorable* in the Western world) exist without a strong feminist movement to lobby for them. Then the tension and the variable outcomes between women's protective policies and individual advancement policies, recognized by Stetson and most popularly addressed by Sylvia Ann Hewlett in *A Lesser Life* (1986), might be explainable in the context of the political cultures and policy processes of different Western states. As the books under review show, the study of women in politics has come a long way. But there is still a long way to go.

DONLEY T. STUDLAR

Centre College

Italy at the Polls, 1983: A Study of the National Elections. Edited by Howard R. Penniman. Durham, NC: Duke University Press, 1987. 216p. $39.95 cloth, $13.95 paper.

The 1983 Italian parliamentary elections resulted in a postwar electoral nadir for the Christian Democratic Party (DC) (32.9%, down from 38.3% in the previous elections).

The Italian Communist party (PCI) maintained, in contrast, a relatively stable 29.9% (30.4% in 1979). Two minor parties fared well: the Republicans (PRI, 5.1% from a previous 3.0%) and the Socialists (PSI, 11.4% from 9.8%). *Italy at the Polls, 1983* analyzes these results.

The extent of the DC's 1983 losses and the reasons for them are examined in Douglas Wertman's chapter on the Christian Democrats as "big losers." Wertman presents extensive discussion of the DC internal reforms under Ciriaco de Mita and examines where the DC lost votes and to whom. Wertman does not overdraw the long-term consequences of the 1983 elections: the DC losses were "A continuation of its crisis of the mid-1970s . . . a result of the combination of the long-standing and profound disillusionment with the DC's rule in Italy and the diminished usefulness of the anti-Communist appeal in 1983" (p. 50). Wertman's claims that extreme changes in the Italian party system are unlikely and that Mita's strategy of renovating the DC did not have enough time to succeed before the 1983 elections have been borne out by the 1987 results.

According to Marcello Fedele the 1983 elections marked crucial but less obvious changes for the Communists. The PCI was able to maintain its 1979 electoral showing in the context of a "new political cycle" (p. 60). In the 1983 campaign the PCI attempted to express a new Communist identity, choosing to (re)present itself as a governing alternative, having rejected the "historic compromise" strategy in 1980. At the same time, democratic centralism as an internal organizing principle became a serious issue. Fedele provides a complicated discussion of internal party struggle over organizational issues and concludes that "the Communists [emerged] from the 1983 elections with no more votes and with the same problems that they have had since they launched the policy of the alternative" (p. 74).

The experience of the PSI in the 1983 elections illustrates how the minor parties in Italy can translate very modest electoral results into significant cabinet positions, as well as the extent to which the DC—as a dominant, but not majority, party—is dependent on them to retain its position in government. Despite Bettino Craxi's 1983 appointment as prime minister, it is still unclear whether the PSI will have any greater role in governing Italy than that suggested by Robert Nilsson in his chapter on the Socialists: "the PSI . . . must be either a subordinate or a destabilizing element in any center-left coalition it joins. . . . The PSI can veto any unwelcome DC initiatives without being able to impose its own will on its partners" (p. 79). Nilsson provides a useful discussion of PSI campaign proposals, and recognizes that despite its modest success in 1983 and ascension to the prime ministership, the PSI has not been able to attract large numbers of new voters, particularly among "the emerging classes" (p. 91). While the PSI increased its share of the vote in the 1987 elections, it is not yet clear from whom those votes were received.

Giacomo Sani's excellent essay on the electorate's "ambiguous verdict" examines the changes in voter choice in 1983 and the reasons for the DC's unusually poor showing. Examining electoral returns on a province-by-province basis, Sani concludes that "changes in party strength in 1983 have not significantly altered the preexisting pattern of vote distribution over the national territory" (p. 26). The record defections among former DC voters did not automatically benefit the PSI. Italian voters still employ Left-Right distinctions when evaluating political parties and are not inclined to move far from their party of previous choice. However, the minor parties are becoming a viable alternative among a particular segment of voters under certain restricted conditions in the context of continued strong core support for the DC and the PCI.

Robert Leonardi, in his controversial chapter on the small parties, claims at the outset that "the 1983 parliamentary election signaled the opening of a new era for the minor parties, an era characterized by a fundamental restructuring of the party system" (p. 100), although not by a major transformation of the electorate. Since the minor parties that gained votes in 1983—the PSI, the PRI, and the Social Democrats (PSDI)—are those that have been essential for the maintenance of a PCI-excluding center-Left coalition, even modest increases in their share of the vote could have an impressive impact on the balance of party power. (Leonardi claims these parties were particularly successful in converting their votes into parliamentary seats.) The implications of the minor parties' 1983 successes are a major issue of disagreement among contributors to this volume. While Leonardi's fine chapter is bound to provoke debate about interpretation and prediction, it will serve as an essential resource for those who wish to know more about the small parties in the Italian system.

Italy at the Polls, 1983 also includes Carol Mershon's solid essay on the role of the Italian labor unions, William Porter's chapter on the mass media, and Joseph LaPalombara's reflective concluding chapter. There are several extremely useful appendixes, including Senate and Chamber district electoral results and Douglas Wertman's concise explanation of "government formation in Italy."

Italy at the Polls, 1983 is somewhat disappointing. Its major failing is that it is not a coherent work. First, it lacks an introductory chapter that would provide a context for understanding the 1983 elections. Had there been a substantive introductory chapter (for example, see Sidney Tarrow's superb introduction to *Italy at the Polls, 1979*), this volume would have been an outstanding work on Italian politics, rather than an uneven collection on the penultimate Italian elections. Second, the volume's intended audience is unclear. For example, Fedele's chapter on the PCI will be incomprehensible to all but those who are fully cognizant of contemporary Italian politics, yet Norman Kogan's background chapter is clearly intended for the uninitiated. However, scholars will still be well served by reading this volume. Like its predecessors, *Italy at the Polls, 1983* is an excellent resource volume, and several of the contributions are essential reading.

KAREN BECKWITH

College of Wooster

Ireland at the Polls: 1981, 1982, and 1987: A Study of Four General Elections. Edited by Howard R. Penniman and Brian Farrell. Durham, NC: Duke University Press, 1987. 275p. $52.50 cloth, $17.50 paper.

This book is part of a national election study series under the general editorship of Howard Penniman, published jointly by the American Enterprise Institute and the Duke University Press. An earlier volume dealt with the 1977 parliamentary elections in Ireland.

The Irish elections are interesting from several perspectives. First, Ireland is the only major country to employ the single transferable vote (STV) system of proportional representation for choosing members of both houses of the national legislature. Second, Ireland has had comparatively stable electoral and political systems, with the two largest parties displaying little ideological difference. Third, the Republic of Ireland's political system has sustained considerable stress in recent years due to serious economic and budgetary difficulties.

Several distinguished analysts of Irish politics contributed to the volume. The book is comprised of chapters by John Bowman, Basil Chubb, John Coakley, Brian and David Farrell, Peter Mair, Joseph O'Malley, and Richard Sinnott, as well as two additional chapters by Brian Farrell. Basil Chubb wrote an appendix on voting procedures, and Richard Scammon compiled an appendix providing electoral data. Howard Penniman's preface provides a useful brief discussion of the Irish STV system in comparative perspective.

The essay by Basil Chubb and the concluding essay by Brian Farrell and David Farrell assume a somewhat gloomy hue in their treatment of the ability of the Irish parliamentary system to cope effectively with economic crises confronting the nation. In Chubb's words, "Although Ireland is still a parliamentary democracy, there are no grounds for complacency about its prospects for remaining so. The democratic mainspring, its representative institutions, does not function well" (p. 207).

The excesses of democracy, the ineffectiveness of corporatist politics, and weakness of the parliament are themes that make these two essays and the volume as a whole of particular interest to those interested in democratic political development. The concluding essay suggests the need for a new constitution, noting the necessity for reform at the administrative, parliamentary, and executive levels. Given the pressures of crisis management, Brian and David Farrell see little chance that such structural reform will actually take place.

The chapter by Richard Sinnott provides an interesting analysis of patterns of electoral support, noting—among other salient points—the absence of a significant class basis to Irish politics for the elections studied. Peter Mair provides a useful discussion of efforts to centralize party control in a political system noted for the importance of localism in electoral politics. Maurice Manning's chapter on women in politics will be of interest not only to students of Irish and electoral politics but also to students of women and politics. The tension between powerful conservative and male-dominated institutions on the one hand and a developing women's movement on the other, suggest that Ireland may be an interesting case study for this issue, particularly in light of such phenomena as constitutional prohibitions against divorce and abortion. John Coakley's contribution on elections to the weak Irish Senate is of interest because Ireland has the only national parliament with a house chosen along occupational and functional lines. All chapters of the volume are competently done.

Unfortunately, most of the book is dated, since only the concluding essay deals explicitly with the 1987 election and its ramifications for the party and political systems. It is a pity that the other chapters could not have been updated, if only to include brief concluding sections on the impact and implications of the 1987 election.

This book, like the series of which it is a part, is a useful addition to the political science literature dealing with electoral and party politics. It also constitutes a significant addition to available literature on the Republic of Ireland.

DAVID E. SCHMITT

Northeastern University

Women, State, and Revolution: Essays on Power and Gender in Europe since 1789. Edited by Sian Reynolds. Amherst, MA: University of Massachusetts Press, 1987. $25.00 cloth, $9.95 paper.

This anthology of readings deals broadly with issues relating to women and power in Germany, France, Britain, and Russia since 1789. The articles are drawn primarily from a set of papers presented at the University of Sussex in 1985. While the editor suggests that "what we did was to take a central notion and to contribute from our different fields a variety of essays—case studies—that would not have been possible say, ten or even five years ago" (p. xi), the "central notion" that animates the collection is never made explicit. Hence, the book contains a series of discrete articles, of a somewhat uneven quality, focusing on disparate eras and aspects of women's lives in European nations. While revolutionary activity is not examined in all the articles, most seem to fall loosely into two categories: articles examining the symbol of woman as a revolutionary figure and articles assessing women as political activists.

The best of the essays included in the volume seek to confront the issue of whether women's presence in political and revolutionary efforts is significant in promoting change or progress. Beginning with an essay on women's politics and sexuality in Delacroix, several of the articles examine the abiding power of woman as a symbol in revolutionary politics. Marcia Poynton explores the symbol of Delacroix's *Liberty on the Barricades* as occupying an uneasy and hitherto unexplored terrain between freedom and license. In a related essay, Sian Reynolds argues that the connection modern French feminism has made with the establishment of the French Democratic Parliamentary Republic in 1789 is based on flawed analysis. Reynolds juxtaposes the symbol of Marianne, a representation of the French Republic, with the actual, unemancipated role of women in the Republic. Reynolds argues that far from liberating women, the French Republic was an agent of their exclusion from the public sphere, following Rousseau who viewed women as a destructive political force. She demonstrates that politicians, historians, and feminists alike have accepted the view, bolstered by anticlericalism, that women were unworthy participants in the Republican polity. Perhaps the most unusual variant on the subject of women and revolution included in this collection is presented in Ulrike Hanna Meinhof's "Revolting Women" which attempts to link media representation of terrorists in West Germany with the press treatment of Greenham Common women in Britain. I am prepared to accept the author's argument that the press may seize on women acting in nonconventional roles (the "second face" of Delacroix's Liberty) and pervert their role in political protest. But it is a far cry from the nonviolent antinuclear efforts of Greenham Common women to the Baader-Meinhof terrorists in West Germany. Beyond making the point that the sensationalistic press distorts gender-related factors by comparing these two political activities—one pacifistic, one terroristic—the article virtually equates the two movements and legitimizes the latter. Is this what the author really intended?

Other articles tackle somewhat less controversial topics. Beryl Williams portrays the Russian revolutionary Alexandra Kollentai as a fighter for women's liberation within a socialist, collectivist society. Her vision of a "new society" apparently has much in common with aspects of contemporary reality in modern-day Scandinavian politics. Kollentai emphasized the importance of women's entering the labor force in order not to have to rely on bourgeois marriage and being free from housework and child care through the provision of alternative collectivized services. Kollentai, however, saw motherhood as a duty, lacking a vision of reproductive freedom as part of her concept of a new life for women. In a very different kind of analysis, Sybil Oldfield argues for a reinterpretation of women's role in the anti-Nazi resistance, stressing the significance of those who overcame their subordinate role in order to seek to save Hitler's victims.

The essay most concerned with the role of women as citizens in the existing state is Anne Stevens, who documents the absence of women from positions of public institutional power in Germany, France, and Britain. While Stevens wonders whether the inclusion of women in greater numbers would bring about meaningful change, she does not draw sufficiently on U.S. data (from the Center for the American Women and Politics at Rutgers) or data drawn from Europe (e.g., Sweden) suggesting that female officeholders do bring a more progressive and ultimately supportive view of women's issues to bear on their public roles and responsibilities. Nonetheless, Stevens argues in an unacknowledged Millsian vein that women would both contribute to state affairs and develop their own talents to a greater degree if their participation increased commensurate with their numbers.

The articles presented in *Women, State, and Revolution* offer the reader important new information and provocative insights about, although they provide no definitive answers regarding, the significance of women activists in modern Europe.

JOYCE GELB

City College of New York

Foreign Aid Reconsidered. By Roger C. Riddell. Baltimore: Johns Hopkins University Press, 1987. 309p. $35.00.

With the problem of massive deficits confronting many developed states, the justification for continued foreign aid to developing countries was bound to become the subject of careful analysis. Riddell deals with this question by reviewing empirical evidence and the moral arguments for and against aid. This book is not only a thorough analysis of the aid issue but it is also a microcosm of much of the debate on the international political economy of distribution.

Riddell notes that the moral case for aid has either been assumed or tautologically defended. In his discussion of the moral argument (some of the reasons include human solidarity, alleviation of misery, and equality) the author includes much of the normative thought on distributional justice. Thus Rawls, utilitarianism, and Christian thought provide a moral defense. Critics of the moral case include Nozick, Hayek, and Bauer. Key to this minimal state view is that governments should not provide aid but that individuals may.

Other critics of the moral case for aid focus on the issue of national interest and claim that aid should be given only when it is in the national interest. Riddell finds these "rightist" views theoretically flawed.

The book also focuses on the economic theory behind foreign aid. The mainstream view is that aid provides additional resources, which allows for accelerated development. Riddell reviews many cross-national quantitative studies for a consensus on the merits of the mainstream perspective. Unfortunately, the conclusions of these studies are mixed. Riddell attributes the lack of empirical consensus to inaccurate data and faulty methodology.

Riddell reviews the debate around the consequences of aid. Critics on the Left (Lappe, Hayter, and dependency theory) claim that aid only protects the status quo and capitalist economies that hurt the poor. The Right (Friedman and Krauss) argues that aid distorts market forces. Obviously aid cannot both help and hurt the market. The Left and the Right do agree that the state is strengthened.

Riddell analyzes several micro and macro aid projects. The conclusion here is that while there is much evidence to indicate the limits of aid, Riddell is not willing to call for an end to all aid. The reasons for the failure of aid are complex: political structures, economic policies, resource use, policy coordination, and bottlenecks. "Part of aid's failure lies in the very nature of underdevelopment" (p. 184).

In the concluding chapter, while Riddell notes that "there is nothing automatic about the link between aid and development" (p. 267), he gives qualified support for continued aid. The justification is largely a moral one combined with the fact that sometimes aid works. Riddell also is critical about the amount of aid usually given: "A trickle of water into a dry hot riverbed will sink without trace" (p. 270).

A deficiency of the book is that there is little discussion of aid from the perspective of the recipient state. It would be in order to analyze how state sovereignty obstructs donor states' ability to create the conditions in which aid would work.

Riddell's analysis is thorough and is likely to be viewed as significant work. Academics should be pleased that this practitioner (Riddell is a research officer at the Overseas Development Institute in London) has a solid grasp of the academic literature, including the normative views on distribution. Riddell's conclusion, however, given the extensive antiaid evidence he cites, is something of a leap of faith.

GEORGE J. CVEJANOVICH

Barry University

Multidimensional Terrorism. Edited by Martin Slann and Bernard Schechterman. Boulder: Lynne Rienner, 1987. 138p. $23.50.

Few issues in recent times have generated as much interest as terrorism. Its popularity as a subject of scholarly inquiry is evidenced by the number of journals and conferences specifically devoted to it and the variety of books and articles published concerning this phenomenon. Given this interest, it is surprising that so few authoritative studies have emerged. Indeed, the best treatments remain the seminal works published over a decade ago by Walter Laqueur and Paul Wilkinson.

The aim of this slim volume, in the words of one of the editors, is to provide "explanations of aspects of terrorism by highlighting and examining several basic theoretical approaches to the subject and offering complementary case studies" (p. ix) and thus presumably to fill a gap in the literature. To this end, six essays addressing the theoretical dimensions of terrorism with five accompanying case studies are offered. A useful appendix detailing terrorist campaigns waged by both governments and nonstate actors since 1945 is also included.

The book is divided into three parts. In the first part "Theory Building and Terrorism," Norman W. Provizer covers familiar ground in tackling the problem of defining terrorism. Robert Maranto examines the rationality of terrorism by employing the economic theory of collective (or "public") goods to describe the goals and motivations of terrorist organizations. Bernard Schechterman's provocative discussion of what he terms "irrational terrorism" is by far the most interesting essay in this part. Schechterman persuasively argues that in places long afflicted by violence, such as Northern Ireland, South Africa, Lebanon, and the Israeli-occupied West Bank and Gaza Strip, a new generation of terrorists has emerged among children who regard violence as a normal if not "automatic" means of societal interaction and communication (p. 24). This development, he concludes, has created a situation where indiscriminate violence, "once politically or religiously motivated, increasingly reflects unthinking behavior or outright hatred unconnected to rational goals" (p. 25).

The second part of the book focuses on more specific theoretical issues, such as counterterrorist strategy, terrorism perpetrated by the state, and the rhetorical significance of the terrorist act. Martin Slann trenchantly delineates the "characteristics, graduations, and variants" of state terrorism directed against its own citizens (pp. 39–42) and the "primitivism and zealotry" of state-sponsored *international* terrorism committed by such countries as Libya, Syria, and Iran (pp. 42–43).

The third part contains four case studies and a concluding essay on the difficulties faced by democratic societies in combating terrorism. It is in this part that the book's main weakness becomes apparent. Despite the editors' ambitious effort to link these case studies to the preceeding theoretical discussions, they fall short of that goal. Leonard Weinberg and William Lee Eubank, however, provide a fascinating examination of the contrasting demographic characteristics of Italian terrorists before and after 1977, which illustrates the manifold differences in the socioeconomic composition of successive terrorist generations. "Over time," they conclude, "the terrorists tended to become younger, less politically experienced and more marginally committed to the groups to which they were affiliated" (p. 93). Edward P. Moxon-Browne's excellent analysis of the social and political alienation that fuels terrorism among the Catholic minority in Northern Ireland is of a similarly high caliber. Of special interest is his cogent discussion of the policy choices available to the British government in view of this situation.

Considerably less impressive are Michael M. Gunter's examination of Turkey's response to Armenian terrorism and Gregory F. Rose's discussion of the mobilization of the masses during Iran's revolution in 1978-79. In fact, the inclusion of Rose's essay in this volume is questionable, given that it has little or nothing to do with terrorism. Finally, William Lasser uses three historical examples (Lincoln's attempts to deal with the secessionist threat of 1861, the internment of Japanese Americans during World War II, and Britain's experience in Northern Ireland) to provide a serviceable overview of the problems faced by democratic societies when confronted by terrorism.

With the exception of a few of the essays (Schechterman's, Slann's, Weinberg and Eubank's, and Moxon-Browne's), the material presented in this volume is disappointingly jejune. It is a book that will appeal neither to the specialist, who will find little that is new or especially interesting, nor to the general reader, who will find the theoretical aspects of the book unnecessarily dense. Both types of reader will probably prefer to turn to the standard, if older, works on this subject or perhaps to the similar but far superior collection of theoretical analyses and case studies edited by Peter Merkl, *Politican Violence and Terror* (1986).

BRUCE HOFFMAN

RAND Corporation

Koreans in the Soviet Union. Edited by Dae-sook Suh. Honolulu: University of Hawaii, 1987. 138p. $17.00 cloth, $13.00 paper.

This volume pioneers the largely overlooked study of Soviet Koreans within the context of a Korean independence movement. Five essays provide a useful introduction to the study of issues and related areas beyond the scope of the book. Throughout the collection the term *Soviet Koreans* is used to identify Koreans who, for one reason or another, left their homeland and settled in territories that became part of the Soviet Union.

"The Korean Movement in the Russian Maritime Province 1905-1922" by Teruyuki Hara is a cogent review of the Korean anti-Japanese movement in the Russian Maritime Province. In his description of the Korean independence movement during this formative period in Soviet history, Hara assumes that Japanese elitist fear of anti-Japanese activities by Soviet Koreans was a significant factor in shaping Russo-Japanese relations. He establishes imperial Japan's perception of a threat and describes the apparent willingness of Tsarist and Soviet governments to suppress loyal Soviet Koreans for the sake of amicable Russo-Japanese relations, thereby portraying Soviet Koreans as victims of bilateral diplomacy.

Haruki Wada's "Koreans in the Soviet Far East 1917-1937" expands the theme of oppressed Koreans, emphasizing fluctuating social status before and after the October Revolution. His well-researched survey includes a significant event ignored by Soviet historians—the forced relocation of Koreans from the Soviet Far East to Central Asia. The description of vacillating Soviet policies and their socioeconomic impact on Koreans, while important, is incomplete. Further research should include analyzing policy reversals in terms of Russian domestic politics. In addition, the Russian exploitation of Koreans as cheap labor and Soviet security concerns about ethnic minorities residing on the USSR's periphery merit more study, especially in light of continued Soviet efforts to develop Siberia and Gorbachev's Vladivostok pronouncement that the Soviet Union is and will remain an Asian power. Excellent companion reading for these concerns is Rodger Swearinger's recent volume, *Siberia and the Soviet Far East* (1987).

The unique history of Soviet Koreans is framed in terms of USSR nationality problems by Youn-cha Shin Chey in "Soviet Koreans and Their Culture in the Soviet Union." From this cultural perspective the author provides an insight into the competition between Soviet assimilation processes and the struggles of minorities to preserve elements of their culture while still achieving social mobility. More importantly, Chey also provides a historical background from which we can develop significant questions. For example, his description of a dilemma encountered by Tsarist administrators—exploiting cheap Korean labor while avoiding Korean economic success that would threaten the social order—raises important questions today as Soviet economic reforms are contemplated: Are current Soviet institutions capable or willing to deal with successes of adaptive, achievement-oriented cultures? Will the slow pace of "Russification" be deemed a necessary impediment to new methods of socialism? To these questions and many others Chey provides an inquiry.

Although the introductory remarks are slightly misleading (the author implies that he will compare Koreans in the Soviet Union with Koreans in Japan), Hidesuke Kimura's "Korean Minorities in Soviet Central Asia and Kazakhstan" compares Koreans with other Soviet nationality groups to yield sound conclusions. Using population data and properly qualified arguments, he criticizes the absence in Soviet literature of the mass deportation of Koreans to Kazakhstan and asserts this action to be the precedent for later deportations of other nationalities, which were also prompted by internal security fears. Kimura categorizes Koreans in the USSR as generally urban dwellers or wealthy farmers who have diligently achieved success in the Soviet system without eschewing Korean traditions. His study should stimulate not only ethnographic analyses that compare degree of assimilation and "success" but also examinations of the implications associated with a shrinking Russian population attempting to control a territory of increasing minorities. A related work that places Soviet policies into a perspective of broader regional goals is Donald Zagoria's *Soviet Policy in East Asia* (1982).

Dae-sook Suh concludes the volume with "Soviet Koreans and North Korea," where he outlines characteristics and activities of Soviet Koreans in North Korea and offers explanations for their lack of influence. The fluctuating fortunes of Soviet Koreans are examined as consequences of Soviet-Korea and Sino-North Korea relations as well as internal factors, such as lack of Soviet Korean organization, ideological differences regarding party constituency, and Kim Il-sung's obsession with *juche* style independence.

Koreans in the Soviet Union is a short compilation that focuses attention on an important but neglected topic while encouraging broader research. Clearly, it does not profess to be all-inclusive. For example, reports of North Koreans' working in Soviet forced labor camps or Soviet Koreans' role in Siberian development are not mentioned (see Allen Whiting's *Siberian Development and East Asia: Threat or Promise?* [1981]. With this understanding of its scope, readers will appreciate this capable introductory inquiry because of its application to a wide range of scholarly interests.

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The Rise and Fall of Italian Terrorism. By Leonard Weinberg and William Lee Eubank. Boulder: Westview, 1987. 155p. $25.00.

Domestic terrorism in Italy during the "leaden years" of the 1970s was unprecedented among democratic polities in the postwar era. Although it is true that terrorism was widespread in many countries in these years, the scale of Italian terrorism was more like that of regimes in the process of democratic "breakdown." Instead, Italian democracy has emerged with a renewed vitality in terms of economic health and the efficacy of state institutions. The authors of this important study examine the role and effect of terrorism in industrialized societies and provide a balanced and intelligent account of the Italian case.

The problem in explaining the causes and decline of Italian terrorism has been complicated by a plethora of self-serving analyses, particularly among Italian scholars. Weinberg and Eubank have instead relied on their own analysis of carefully gathered data. Most studies of Italian terrorism have been either highly speculative or apologetic of the motives of the terrorists. Joseph LaPalombara's recent *Democracy Italian Style* (1987) goes so far as to lay a large portion of the responsibility for the growth of domestic terrorism in Italy at the door of an indulgent press and intellectual community. Weinberg and Eubank break new ground by focusing their study on survey research and empirical analyses of many factors—both within the Italian political system and among the terrorists themselves.

The authors shed light on three related and significant areas of comparative study: a definition of domestic terrorism, a study of the democratic polity in transition and under stress, and an objective portrait of Italian politics.

The authors combine cultural and historical analysis with the use of multiple regressions and biographical data on the terrorists (2,512 individuals). Terrorism perpetrated by Italians against their own state can be distinguished by two periods: the early period 1970–76 and the late period 1977–84.

The early period was dominated by neo-Fascist violence that was most often aimed at provocation, typically indiscriminate as to its victims, most deadly in terms of the volume of deaths, and generally secretive and privatized. The late period was dominated by leftist violence by a great number of organizations, most notably the Brigate Rosse, or Red Brigades. This revolutionary terrorism was more public in claiming responsibility, usually chose its victims carefully in relation to their "political crimes," and exceeded the neo-Fascists in the volume of activities and membership. Both types were widespread but exhibited concentrations within certain regions of Italy and significant characteristics relating to the socioeconomic background and upbringing of the perpetrators.

These arguments and ultimately the authors' conclusions, are based on layers of empirical analysis. The research is persuasive, but greater sensitivity to the *spettacolo*, or "drama," character of Italian politics would have been helpful.

Why did this happen in the 1970s, who were the terrorists, and how did the Italian state survive? While recognizing numerous influences, Weinberg and Eubank decline to be bound by explanations seeming to say that (1) Italian society and culture is violent and polarized and (2) the state is too inefficient and disorganized by multiparty conflict, corruption, and patronage to administer to society. Such explanations would be simplistic and inaccurate.

Moreover, analysis of the type of violence showed that a significant part of the second (1977–84) wave was a kind of warfare between the terrorists and the state. A large proportion of the attacks were aimed at the legal establishment or security agencies or were acts of revenge carried over from the first wave.

The authors further discovered that members of Italian terrorist organizations could not be easily categorized. Italian terrorism was widespread and did not correspond so much to socioeconomic status and upbringing as to political ideas and to a lack of consensus endemic to the Italian polity. The authors found a strong correlation between ideas and the commitment of the terrorists. Terrorist organizations were very much a reflection of the splintering of the major political parties, so that "one could do worse than to consider their groups as miniature versions of right- and left-wing European political parties" (p. 89).

Implicit in this study but partially overlooked is the linkage to the historical lack of consensus within Italian politics and the effect of regionalism on the national identity. The strength of terrorism also reflects the disagreement among the Vatican, the liberal center, and the Socialist tradition in Italy dating to the Risorgimento. Finally, the authors seemed to underestimate the possibility that the Italian Communist party's gradual move to the center may have encouraged terrorism through frightening the far Right and frustrating the far Left simultaneously.

As the authors analyzed the response of the state they concluded that certain techniques, particularly the state's use of "repentant" terrorists and liberalized arrest procedures, were efficient in putting down terrorism. In fact, the state response reflected the wishes of a people tired of the violence and polarization. With the kidnapping of Aldo Moro the terrorists crossed the Rubicon and, as the authors imply, may have thereby induced greater democratic consensus. The irony is that Italian terrorism that sought to destroy the liberal democratic state may have abetted the growth of a strengthened Italian center.

Weinberg and Eubank regrettably avoid going into greater detail on the emergent stability of democracy, if not of individual governments, in Italy. Significantly, they discovered some useful lessons Italy learned in the process of controlling terrorism in an environment that could have easily called for the extreme contraction of civil liberties. They conclude that terrorism did not come close to defeating Italian democracy. It was ultimately public support of the state that strengthened its ability to decapitate and defuse terrorist organizations.

This small book is an innovative and provocative contribution to the field and will be useful for the classroom and specialists alike—and all interested in the evolution of democracy in industrialized societies.

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INTERNATIONAL RELATIONS

The Strategic Defense Initiative: Shield or Snare? Edited by Harold Brown. Boulder: Westview Press for Johns Hopkins Foreign Policy Institute, 1987. 297p. $24.95 paper.

SDI: Has America Told Her Story to the World? Report of the IFPA Panel on Public Diplomacy. By Dean Godson. Washington: Pergamon-Brassey's for the Institute for Foreign Policy Analysis, 1987. 73p. $9.95 paper.

In his March 1983 "Star Wars" speech, President Reagan reopened the doctrinal "closet" into which the issue of strategic defenses had been left since the signing of the 1972 antiballistic missle (ABM) treaty. This renewed doctrinal debate on the utility and feasibility of strategic defenses has had to attempt to overcome a number of new hurdles, such as explaining what the Reagan administration's strategic defense initiative (SDI) program actually is; whether its goal is a nationwide "impenetrable shield" or only point defense of intercontinental ballistic missle command and launch facilities; and what its implications are for U.S. domestic politics, NATO alliance relations, and superpower relations.

The two books under review join a publishing growth industry of books and articles on SDI, though former defense secretary Harold Brown asserts at the beginning of his collection that "the issues and arguments have not changed much since the spring of 1983" (p. 1). He contends that there is "the need for a substantial U.S. research and development program on SDI," with the only real question being "the appropriate level and nature of an R[esearch] and D[evelopment] program" (pp. 4–5).

The 10 tightly written papers in his collection are part of the results of a Foreign Policy Institute study group on the issue of military uses of outer space, some of which have already been published elsewhere. Though sensibly divided into three functional parts based on "thinking" about SDI, its feasibility, and its desirability, the contributions in each part tend to be uneven in their approach and range of assessment.

Alternatively, four of the papers can be read together as a well-outlined overview of the nature and implications of SDI. Stephen J. Hadley provides a brief but concise introduction to the nature of SDI, while Congressman Dave McCurdy argues that Congress will continue to fund SDI as a high-priority project, though not above 1987 levels (p. 190). With regard to the Soviet Union, Bruce Parrott suggests that it would be prudent to pursue ballistic missile defense research while scaling down SDI if the latter's main value is "as a hedge against unpredictable Soviet behavior and as a bargaining chip in superpower arms negotiations" (p. 225). And lastly, the late Robert E. Osgood sets out the implications of SDI for U.S.-European relations as seen from this side of the Atlantic.

For those concerned with SDI's feasibility, Brown's own paper argues that defensive technologies for area defense appear cost-effective in the near term (10–15 years), though defense of populations through the year 2010 and beyond looks poor (p. 138). As to the fiscal and economic implications of SDI, Barry M.

Blechman and Victor A. Utgoff suggest that the "opportunity cost" of deploying a comprehensive strategic defense system is possible if the additional revenue could be raised and alternative potential benefits are foregone (pp. 178–79). And with regard to SDI's impact on U.S. foreign policy, Robert W. Tucker argues that a transition to strategic defenses would "in all likelihood leave U.S. foreign policy substantially where it would be without SDI," with the greatest potential effect being "to reassert the primacy of the United States and the Soviet Union in the international system" (p. 96).

The remaining papers consider SDI in terms of the national security traditions in the U.S. ethos (Michael Vlahos), the technological challenge to superpower geopolitical parity (George Liska), and an assessment of the "rationalistic view of the tradeoffs" (Richard K. Betts).

Dean Godson's slim volume, too, supports a continuation of the SDI program, though it neither explicitly states reasons for such support nor suggests "precisely what the objectives of SDI should be" (p. 50). Based upon the findings of the Institute for Foreign Policy Analysis Panel on Public Diplomacy, the apparent purpose of this special report is to review the Reagan administration's "selling" of the SDI program to Western Europe, with a view to offering policy choices for countering adverse Soviet propaganda while encouraging increased allied support. These include such questionable proposals as the necessity "to create an N[ational] S[ecurity] C[ouncil]-based SDI High Command" (p. 51) to develop a coherent SDI public diplomacy strategy toward Western Europe and other key countries elsewhere in the world. Such a "high command," with its chairman purportedly to hold cabinet rank, would "launch and monitor all Public Diplomacy programs related to SDI," including the efforts of the United States Information Agency–United States Information Service missions overseas (p. 51).

By contrast to his reference to "continued single-minded Soviet opposition to the SDI" (p. 66), Godson's volume is a call for single-minded support of SDI. Through more effective use of public diplomacy Godson hopes to achieve "any substantial changes in European popular or elite perceptions" toward SDI that might be possible (p. 67). He suggests targeting European national "issue-oriented" concerns to gain such support and goes so far as to name groups and even specific individuals in France, Britain, and West Germany from whom support should be sought (pp. 59, 63, 66). Such a proposal suggests that the United States' European allies would support SDI if only they recognized their own best interests.

As a proposed game plan Godson suggests that the United States must (1) accurately define the concept-product (SDI), (2) ensure that the campaign has the full support of the key members of the administration, and (3) promote SDI overseas with broadly based support at home (p. 50). With regard to the first two objectives, he is basically correct as to the administration's confusion in defining SDI and its goals. But in the case of the third one, Godson appears to have missed a key point, namely that the administration's pursuit of overseas support can and has been seen as *primarily* intended to influence opinion within the United States, specifically the congressional legislators, based upon broad alliance participation.

In comparison, the collection edited by Brown would encourage a continuation of SDI research and development within the "narrow" terms of the ABM treaty, while leaving considerations of production and deployment of any strategic defenses for the future. Godson's report, on the other hand, calls for enhanced and possibly intrusive administration efforts to promote SDI in Western Europe, based upon the creation of a centralized command of cabinet-level policymakers, though with little apparent concern for its substance or goals.

Evidently written with U.S. audiences in mind, these books offer foreign observers, first, a sense of the intensity that the SDI issue has attained among at least two Washington-based think tanks and second, a sense of the nationalistic "window" through which the United States' allies are seen. Both singularly fail to come to grips with allied concerns about SDI and its widely perceived destabilizing impact on both intraalliance and superpower relations, even during its current research and development stage, despite repeated references to European anxieties and fears over SDI.

But are the Europeans (as well as other U.S. allies) unreasonably fearful of SDI's destabilizing nuclear deterrence, decoupling U.S. and European security interests, limiting NATO nuclear and conventional defense options, and so on? The lack of a European perspective or perspectives only conjures up "wimpy" images of allied misgivings over SDI. But such suggestive images cannot cover over the Reagan administration's own confusion as to SDI goals, Weinberger's NATO "invitation" to participate in the SDI program officially (with its "acknowledgement" deadline), or administration "sales" tours to Europe with the inducement of potentially lucrative SDI research contracts upon signing the official participation memorandum.

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The Federal Republic of Germany and the European Community. By Simon Bulmer and W. E. Paterson. Winchester, MA: Allen & Unwin, 1987. 276p. $39.95 cloth, $17.95 paper.

This book explains the role of West Germany in the European Community. The authors argue that European policy is conditioned by the same factors as shape domestic policy. As a result they present a detailed and comprehensive volume offering analyses of Federal German policy-making processes, interest groups, parties, public opinion, the legislature, the *Länder,* and West Berlin, in relation to the European Community. The book also provides case studies of West Germany policy on the Regional Fund and the European Dimension of its relations with France and East Germany. The authors claim that one cannot make sense of West Germany's European Community relations with either a power politics or a neofunctionalist approach. Accordingly, their study is premised on an interdependence perspective in which governments remain central but are viewed as enmeshed in a complex network of relations with transnational, state, and nonstate actors.

Critical to their analysis is the concept of "policy style." By this they mean the character of the government's approach to problem solving and its relationship with socioeconomic forces in West Germany. The most important source of this style is the constitutional arrangements of the Basic Law. According to the authors, West Germany is a "vague" actor in the European Community because of the institutional pluralism that has been built into its formal political structures. They refer, for example, to the federal political system, the independence enjoyed by the federal bank and the tradition of ministerial autonomy that exists in West Germany. Such features, the authors argue, combine with a permissive consensus towards the European Community in society at large to produce a distinctive form of policy making. The policy style of West Germany is decentralized and routinized. Its consensual character permits most European Community issues to be treated as technical, rather than political, problems. Further, this process gives rise to very little active, coordinated policy except in response to crises.

As a consequence of this policy style, the authors argue, West Germany is poorly equipped to assume a political *Vorreiterolle* (leadership role) in the European Community compatible with its current economic status. However, its elites also are not particularly interested in assuming such a role. The reasons for this are quite clear. First, trade in general is of critical importance to the country's economy. The German economic recovery of the 1950s was premised on free access to Western European markets and its health is dependent on continued access. Thus as it presently exists and insofar as it embodies elements of the "social market economy" ideology said to account for West Germany's economic success, the European Community constitutes an attractive arena of economic competition. Secondly, the European Community also provides an attractive environment for political cooperation in that it allows West German government to participate in European diplomacy in a manner that is neither offensive nor threatening to non-Germans or Germans who are mindful of the catastrophe inflicted upon Europe by the "national" imperatives of Nazi foreign policy. The existence of the West German government in its present form, therefore, is a product of, and dependent on, the same political, ideological and economic developments that gave rise to the European Community. Rather than seeking a leadership role that might place this environment at risk, West Germany is content to play the *Musterknabe* (model boy) of the Community, seeking only a single market within the European Community and freer trade between it and the rest of the world economy.

Of course, the authors note that below the level of grand principle, West Germany is not always a model of *communautaire* behavior. For example, the fragmented policy style of the government has permitted the agriculture Ministry to defend certain price levels for cereal production even though this slows the pace of Community reforms and is at odds with the social market philosophy espoused by the Finance and Economics ministries. Further, West Germany's opposition to Community policies that have an interventionist character has been more sustained when, as in the case of steel, they are seen as undermining a competitive edge enjoyed by the relevant sector of its own economy. Finally, not all moves towards closer European integration are welcomed by all elements within the West German government. The environmental proposals of the recent Single European Act, for example, not only bother the Economics Ministry and its industrial clients, but also pose problems for the jurisdiction of the *Länder* on such issues.

This is a very good book in the sense that it provides a great deal of information about West Germany and the European Community. Its scope is broad, ranging from the early days of the Coal and Steel Community up to the European Community reforms of 1986 and the new issues of high technology development and service industries. There are prices to pay for this detail, however. At times the authors present very dense narratives to set contexts for rather small points, for example, in their discussion of the European Research Cooperation Agency research project and West German policy. Finally, the book should offer a more extensive defense of its theoretical perspective. The authors' research may have led them to adopt the interdependence perspective in preference to realist, neorealist, Marxian, or functionalist approaches. Their study of the forces that shape domestic policy in West Germany reflects this choice but rarely makes the case for it.

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Big Brother: The Soviet Union and Soviet Europe. By Hélène Carrère d'Encausse. Translated by George Holoch. New York: Holmes & Meier, 1987. 332p. $39.50 cloth, $24.50 paper.

Soviet influence in Eastern Europe, acknowledged as a principal factor in generating the cold war, continues to be routinely—almost ritualistically—denounced in the West. Although the denunciations may in most instances be richly deserved, they are a poor substitute for efforts to analyze and understand the Soviet Union's relationship with this complex and unlucky region. It is a daunting challenge for an individual author to attempt an explanation of Soviet policy in Eastern Europe from the Second World War to the present day. Such an unwieldy subject is more often confronted in multiauthored and edited works. Hélène Carrère d'Encausse has, however, in *Big Brother: The Soviet Union in Soviet Europe,* accepted the challenge of single authorship with results that on balance constitute a very impressive achievement.

Originally published in French in 1983, the book is now available in an English translation that contains an updated afterword by the author. This addition is particularly important, given the fact that the original version's publication predates the selection of Mikhail Gorbachev as general secretary of the Soviet Communist party.

Divided into three sections, *Big Brother* seeks, in part 1, to describe the ways and means by which Stalin's Soviet Union sought and achieved control over Eastern Europe both during and immediately following the Second World War. Carrère d'Encausse then turns in Parts 2 and 3, to the subject of the repeated attempts by the peoples of Eastern Europe to resist Soviet hegemony and the nonetheless largely successful efforts of the Soviets to overcome resistance and devise new ways to build a "socialist community."

Carrère d'Encausse debunks the "Yalta myth" that at the 1945 conference the West handed Eastern Europe to the Soviet Union. Rather, Yalta "merely confirmed previous decisions and a situation that favored Soviet demands" (p. 31). This is not to say that Stalin had a detailed plan for the subjugation of Eastern Europe. He was not empowered with the capacity to see into the future. Stalin *was,* however, pragmatic and adaptable and prepared to take political advantage of the Soviet military presence in Eastern Europe. And Stalin feared the West. Carrère d'Encausse suggests that U.S. atomic capability conveyed an unmistakable signal to the Soviet Union that the balance of power had shifted against it, and that the United States sought strategic dominance. Stalin, therefore, no longer felt bound by his wartime commitments, and he greatly intensified efforts to solidify, and indeed institutionalize, the Soviet hold over Eastern Europe.

Stalin's methods of ensuring political uniformity were often harshest when directed at loyal communists who were nonetheless suspected of national deviationist tendencies. In dealing with this issue, *Big Brother* suffers from an organizational defect in that Carrère d'Encausse chooses to discuss the purges of the late forties and early fifties *before* she describes the Soviet-Yugoslav confrontation, an event that was itself largely responsible for provoking the bloodlettings in Czechoslovakia, Hungary, Bulgaria, and elsewhere. Despite the somewhat confusing chronology, however, Carrère d'Encausse provides us with a depressingly thorough description of the lengths to which Stalin was prepared to go to ensure political homogeneity in Eastern Europe.

In Part 2, entitled "The Fratricidal Empire," Carrère d'Encausse takes up the numerous challenges to Soviet authority that range over the period from the late forties to the eighties and that at one time or another have involved every country in Eastern Europe save Bulgaria. Here she recounts not only the succession of sometimes dramatic crises that have plagued the Soviet Union but also the continuous effort on the part of the Soviets to pursue a policy of "normalization" combining violence and limited concessions.

The attention paid to Eastern Europe in the West, and perhaps especially in the United States, tends to be sporadic and self-serving. It becomes riveted on the region only in times of upheaval as politicians and others seek to exploit discontent in order to embarrass the Soviet Union. What is generally overlooked and what becomes the focal point for Carrère d'Encausse in Part 3 of *Big Brother* is the unspectacular but essentially relentless process engaged in by the Soviet Union to construct an integrated and permanent socialist system in Europe, similar in key respects to the Ottoman Empire of the Janissaries. Utilizing principally the instrument of the Warsaw Pact as an integrating tool, the Soviet Union's long-range objective has been to create the "habit of living together" (p. 294). Despite repeated setbacks, the Soviets can claim a large measure of success in reaching their goal.

Unfortunately, Carrère d'Encausse's generally scrupulous analysis is marred by her conclusion, in which she attempts to place the Soviet role in Eastern Europe in the context of global politics. This proves to be altogether too ungainly a subject for the few pages devoted to it, with the result that the reader is treated to little more than ominous speculation about the fates of Yugoslavia and Turkey, punctuated by unexamined quotes from Jean-Francois Revel and Sir Halford Mackinder.

The afterword, written especially for the English language edition, bears marks of having been rather hastily put together (e.g., Brezhnev died in 1982, not 1983). But Carrère d'Encausse raises some very pertinent questions about the relationship between *perestroika* in the Soviet Union and the imperative of preserving the socialist system in Eastern Europe. Developments as recent, for example, as those in Bulgaria in 1988 suggest that Gorbachev's Soviet Union, despite the necessity of efforts at internal reform, will continue to keep a very watchful eye on the pace of change in Eastern Europe.

SCOTT MCELWAIN

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Trading Technology: Europe and Japan in the Middle East. By Thomas L. Ilgen and T. J. Pempel. New York: Praeger, 1986. 203p. $37.50.

Trading Technology is an examination of the trade in high technology between four industrialized countries – Britain, France, West Germany, and Japan – and the Arab states of the Middle East. The book is an outgrowth of a project organized by the Office of Technology Assessment (OTA) designed to examine the policies of major suppliers of technology to the Middle East. The book expands on the OTA report "Technology Transfer to the Middle

East" (1984) because the authors concluded "that our research on the policies of technology suppliers offered broader analytical insights into the dynamics called the generic process of technology transfer. It also permitted us to link scholarly understanding about the unique national features of foreign economic policies to the particular characteristics of technology trade" (p. ix–x).

The book defines technology very loosely as any scientific knowledge that can be put to a socially useful purpose. They examine technology trade in the Middle East within four stages of transfer: technological planning and project design, infrastructure projects and transfer, large-scale industrial imports, and small-scale transfers and specialized technologies.

The authors present a brief overview of trade between the Middle East and the major industrial countries and then concentrate on six different product areas as a representation of technology transferred: nuclear plants, machinery, telecommunications, medical equipment, aircraft and air transport equipment, and arms. As one might expect, market share for products in these categories is specialized and varies over time. Germany, for example, is the leading supplier of medical equipment to the region, France and Britain are the leading suppliers of telecommunications equipment but are now being overtaken by Japan.

The book then devotes four chapters to how the four countries have approached technology trade with the Middle Eastern nations. Each chapter explores the historical context of relations between the industrialized nation and the Middle East, explores trade and investment initiatives by government and the private sector in that country, discusses the activities that support trade and investment policy as it applies to technology transfer, and assesses the national strategy of relations toward the region and accounts for distinctive national features. The chapter on Japan, for example, notes Japan's dependence on oil from the area and the recent origin of its ties to the Arab nations. It briefly discusses the role of government in Japanese industrial policy including the Ministry of International Trade and Industry, the Japan External Trade Organization, and the Ministry of Finance. Japan's success in developing technological sophistication is also attributed to its conglomerates *(keiretsu);* its banking structure, particularly the Bank of Japan; and the major trading companies (*sogo shosha).*

Then follows a discussion of the Euro-Arab dialog and its complexities and difficulties. The book concludes with a discussion of the need for planning among technology importers, specialization among suppliers, the role of government in trade policy in supplier nations, and potential changes in trade relationships between developed nations and the Middle East.

The book is almost an abbreviated cross between Peter J. Katzenstein's *Small States in World Markets* (1985) on the one hand and Chalmers Johnson's *MITI and the Japanese Miracle* (1982) and M. Y. Yoshino and Thomas B. Lifson's *The Invisible Link* (1986) on the other, but with a Middle Eastern flair.

For those interested in this niche in the trade literature, the book provides a quick overview. It also exemplifies the potential strengths and weaknesses of various types of industrial policies as they apply to trade promotion. But as with so many things in life, strengths are also weaknesses. While brevity makes for nice overviews it does not allow the authors the opportunity to tell us much that we have not been told before.

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The Limited Partnership: Europe, the United States, and the Burdens of Alliance. By Josef Joffe. Cambridge, MA: Ballinger, 1987. 225p. $29.95.

In this analytically rich and eloquently argued book, Josef Joffe who has published widely in the area of transatlantic affairs and who is currently foreign editor of the prestigious Munich daily, the *Süddeutsche Zeitung,* deals systematically with the major past and present problems of the alliance, especially the area of security policy.

In the introduction and chapter 1 Joffe provides a succinct and convincing argument of why and how the alliance suffers from a transatlantic rift. Also addressed are the consequence of a changing European and global order the United States finds it difficult to adjust to, the differing hopes and aspirations the alliance partners attached to the détente processes of the 1970s, differing assessments of the nature of the Soviet threat and how the alliance should respond to it, and deeply rooted and divergent historical experiences and cultural proclivities. Joffe's analysis is particularly cogent when he insistently points to the singularity of the Federal Republic's historical, institutional and political position in the alliance—the result also of Germany's peculiar geostrategic and historical middle position in Europe—which explains in large measure why the problems that have plagued NATO were always felt more acutely in the Federal Republic than elsewhere in Western Europe. In stressing the special problems, anxieties, aspirations, and resentments that accompany West Germany's membership in the alliance, Joffe also provides a persuasive set of reasons why there is no "Western Europe" as such and why a full understanding of what ails NATO must recognize that West European states follow divergent foreign policy interests. This places an intra-West European rift on top of the transatlantic rift and compounds the problems the alliance faces on the eve of its fortieth anniversary.

A large but sometimes hesitantly argued theme that runs through the entire book—especially chapters 2-4 is that NATO must reconnect politics and strategy and develop a political-strategic posture that is sensitive to the changing political circumstances of Europe and adequate to the task of meeting the challenges of the post-Intermediate Nuclear Forces era, especially in the area of East-West arms control where Soviet diplomatic dexterity is pronounced. Hopefully, this reconnection could help also in restoring broad domestic support for the nuclear policies of the alliance, support that has become increasingly corroded (especially in the Federal Republic) as a consequence of ill-considered U.S. arms control policies and strident cold war rhetoric.

What makes Joffe's book an especially welcome addition to the spate of books on the transatlantic alliance is its sustained emphasis on the political rather than the purely military-strategic sources and consequences of the tensions within NATO. This is done deftly, interweaving political and strategic considerations in a way that demonstrates—more through implication than exposition—the damaging consequences of U.S. nuclear and arms control policies in Europe during the Reagan years. There is no need to lay every malaise of the alliance at the doorstep of Washington administrations. (The Europeans contribute their fair share as well.) The alliance has survived so many crises that one feels grateful that Joffe sounds less alarmist than many others who view the present state and future prospects of the alliance (especially its German-U.S. backbone) with growing concern. Even so, not everyone would share Joffe's rather sanguine conclusions—for example, that NATO has done its job well and that with respect to nuclear versus conventional deterrence, "four decades of ultrastability is an impressive argument for the status quo" (p. 166). It is here perhaps that Joffe lets lapse his otherwise persistent stress on the primacy of politics over strategy. For NATO was never intended to serve narrowly defined Western security interests—if that had been its primary purpose, the success of the alliance might indeed be evaluated in equally narrow terms of how it sustained deterrence—but rather was intended to stabilize the postwar European political order by containing both the Soviet Union and the Federal Republic. The real question (and Joffe provides ample evidence for its cogency) is not so much in what ways the alliance can continue to uphold deterrence but whether it can transform its traditional role of double containment and adjust it to the more complex circumstances and challenges of the 1990s and beyond.

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Moral Paradoxes of Nuclear Deterrence. By Gregory S. Kavka. New York: Cambridge University Press, 1987. 243p. $34.50 cloth, $12.95 paper.

This study tries to answer the question, "Which possible nuclear deterrence policies are morally superior to which other policies, and for what reasons?" (p. 1). Disclaiming any comprehensive purpose, Gregory Kavka does not treat the use of nuclear weapons. Kavka believes that achieving nuclear disarmament is a moral imperative. Reaching the goal, in his view, will not be easy and may not even be likely. This is the case not only because of the entrenchment of nuclear weapons but also because of the existence of nuclear deterrence paradoxes.

Relying on Thomas Schelling for the identification of several paradoxes, Kavka is chiefly interested in moral paradoxes overlooked by strategic analysis, for example, paradoxes arising out of the moral status of the defender's intention to retaliate. Such "evil" intentions may actually prevent greater wrongs, notably nuclear first strike or nuclear blackmail. The book is replete with carefully delineated moral ambiguities that nuclear abolitionists have failed to address or have dismissed too readily. The study is addressed much more to nuclear pacifists, especially unilateralists (who may be rare), than to ideological and security partisans of nuclear deterrence. One misses references on the prudential side to the Harvard Nuclear Study Group, Ernest Lefever, and Wolfhart Pannenberg and on the apocalyptic side to the American Methodist bishops, who have gone beyond the U.S. Catholic bishops to deny any legitimacy to nuclear deterrence.

The book is composed largely of articles appearing previously in philosophy journals. Well-integrated, the volume addresses, inter alia, deterrence and utilitarian moral theory, the dilemmas of nuclear protest, and nuclear coercion. After concluding that unilateral nuclear disarmament has risks that disqualify it from being a lesser evil as compared to minimum nuclear deterrence, Kavka examines world government as a possibly lesser evil than a minimum deterrence regime. A Hobbesian scholar, Kavka reviews Hobbes' argument against anarchy and his views on international relations as a run-up to analyzing different variants of the "Nuclear world government" argument. To Kavka, the "proliferated world" version of the argument is more persuasive than the present world condition in which only a few states have nuclear weapons. The point is recalled that most members of the nuclear-armed crowd would be highly unlikely to have second-strike capability, thereby undermining the supposed effective equality stemming from possession of nuclear arms.

Kavka might have noticed that both the nuclear mob spectre invoked by Albert Wohlstetter over a decade ago, which is still exploited by antiproliferation sensationalists, and the seldom-replicated proliferation-favoring advice once offered by Kenneth Waltz in the name of stability have not been realized. The antiproliferation regime has been more effective than even many of its partisans have been willing to admit. The book and cited sources do not mention the leading proliferation challenge, the potential spread of covert bomb states in emulation of Israel.

Identifying well-known problems of gaining surrender of national authority to a world regime, the chapter on world government concludes that its achieving is not necessary to securing nuclear disarmament. World government may not be possible without nuclear disarmament. There are lessons in these findings for the world order school and other antistatists.

No comfort is offered to the Strategic Defense Initiative. Using game matrices to analyze strategic defense, a chapter concludes that even a "pure defense of the innocent" could be morally flawed, because "such an act could protect the guilty, deflect harm onto other innocents, make aggression by the defended party safer and more likely, or be ineffective as a means of defense" (p. 159).

After disposing to his satisfaction of a "dangers of transition" argument, Kavka places his final trust in "bilateral mutual nuclear disarmament to the lowest level providing a secure second-strike capacity to each side (i.e., minimum deterrence)" (p. 206). For the "very short run" he urges the nuclear giants to adopt a nuclear freeze (undefined), a comprehensive test ban, a ban on space weapons, a continued bar on strategic defense, "large" reductions of nuclear stockpiles, elimination of first-strike strategic weapons, and a no-first-use pledge (p. 207). This highly ambitious agenda may unsettle arms controllers and gradualist disarmers waiting for Strategic Arms Reduction Talks progress, while pleasing abolitionists, whose haste has been questioned in the book. The study has a built-in paradox. Nonetheless, the volume is a distinctive contribution to nuclear discourse.

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Book Reviews: International Relations

Making and Marketing Arms: The French Experience and Its Implications for the International System. By Edward A. Kolodziej. Princeton: Princeton University Press, 1987. 518p. $55.00.

France, Kolodziej tells us, is the third largest arms exporter in the world, after the Soviet Union and the United States. Arms represent perhaps only 4%-5% of its overall exports, a ratio comparable to that of the United States and one-third that of the Soviet Union. But on a per capita basis France now sells three times as much as either of these. Why? What impels it? What does the French sales effort mean in terms of the burgeoning militarization of the third world and the prospects for control of the arms trade?

The importance of Kolodziej's book lies in providing answers to these questions in the broad context of historical dynamics and international relations theory, as well as in its impressive marshaling and presentation of data and its detailed analysis of the French military-industrial complex. The anarchic Hobbesian self-help system impels states that claim sovereignty and independence to arm themselves against potential enemies. Yet the revolutionary and expensive technology of our times make it less and less possible for states other than the superpowers to do this on their own, to truly remain sovereign independent entities in today's world. France, however, has seized upon arms sales as a means to maintain an arms industry capable of satisfying what successive French governments have seen as necessary for real independence. Virtually all parties accepted the Gaullist rationale that France's own deterrent, its *force de frappe*, was necessary in avoiding an unreliable and inadmissible U.S. hegemony. Building it, however, put a budget squeeze on conventional forces. Withdrawal from empire provided some relief and allowed France to mend its third-world relations. French policy of sales to third-world countries then helped finance continued conventional weapons development and production. Moreover, it has been rationalized across the years as serving a dual purpose: (1) by giving third-world countries an alternative to purchases from the superpowers it allows them, too, to avoid falling under superpower domination; (2) during a period when superpower conflict is muted by the nuclear standoff, it helps decentralize power in the world, creating regional, competing centers. If NATO standardization and coproduction has remained something of a chimera, this, too, is because of the imperative of independence.

Moreover, the French Fifth Republic appears as a result to have successfully avoided—even reversed—the choice between guns and butter. Arms production and sales became a way of stimulating a thoroughgoing modernization of the French economy, helping to make it competitive in a world of rapidly changing technology, to make it meet what Jean-Jacques Servan-Schreiber called—25 years ago—the "American Challenge." The military-industrial complex and its exports thus became essential to the French government's stimulation of employment and welfare. The result of all this is that only extraordinary circumstances will bring a diminution in French resolve to continue arms sales. More third-world states desirous of attaining some measure of independence and modernization may well, Kolodziej argues, emulate France. The imperatives of the anarchic, self-help state system drive states to amass weapons. But to this Kolodziej adds the imperative of modernization: the fact that France's use of weapons production and sales has accompanied the extraordinary transformation of French society in the last quarter of a century has not escaped the observation of decision makers in major new states, and once one state enters into the process, through purchase, coproduction, or licensing, its neighbors follow suit.

Kolodziej argues persuasively that the progressively concentrated arms industry has been crucial to French modernization. But in the course of the process France has created a set of interlocking institutions that are geared to arms sales, divorced from a largely acquiescent public opinion and a generally uninformed legislature, and supported by both the Right and the Left. The Socialists talked about "moralizing" arms sales before coming to power in 1981 but made little change in policy.

Twenty years ago George Thayer's *War Business* (1969), sounded a warning, chronicling the change in the arms trade from the period after World War II, when surplus and private sales dominated the field, to the spread of government sales, based mostly on economic criteria. A decade later, updating Thayer, Andrew Pierre's *Global Politics of Arms Sales*

(1982) stressed the political element in the accelerated growth of arms sales in the 1970s, arguing that they might or might not contribute to local balances or to local conflicts but that they were largely a function of the end of the bipolar postwar world and of the diffusion of power: new states wanted armies and weapons. Arms sales, moreover, represented the continued competition between East and West for influence in the non-Western world. The economic benefits to sellers were less than generally believed, however, and since four sellers dominated the market, a cooperative international endeavor in which recipients worked with sellers to limit sales might have been possible. Kolodziej's new work, in contrast, demonstrates the enormous internal barriers to working out such cooperation and underlines the fact that the complex interdependence of our current world must still be mediated through the institution of the sovereign state. French policy will never run smoothly: the export market shrank in 1987, modernization of the *force de frappe* and construction of the nuclear carrier *Charles de Gaulle* and a potential second one have put a new squeeze on conventional forces. The costs of new equipment continue to rise. The Intermediate Nuclear Forces treaty is taken by some French as the sign of further U.S. unreliability. Franco-German cooperation, while largely symbolic so far, may increase, though differing geostrategic interests constantly interfere. But Kolodziej's first-rate and important book, marred only by some repetitiousness, provides the deepest insight so far into what lies behind French policy and gives us a model to analyze the arms policies of other countries. The work is essential to anyone who wants to go beyond simple condemnation of what has in the last quarter century become an important part of international relations.

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Exploring the Stability of Deterrence. Edited by Jacek Kugler and Frank C. Zagare. Boulder: Lynne Reinner, 1987. 168p. $18.00.

However much they may differ on the wisdom of particular policies, both "peace studies" and "security studies" analysts are widely agreed on the importance of the subject of this edited book: how to maintain and increase strategic stability and decrease the likelihood of a nuclear war. In *Exploring the Stability of Deterrence* editors Jacek Kugler and Frank C. Zagare contribute to the extensive literature that uses formal models and game theory to explore the implications of alternative assumptions and force structures for strategic stability. As such, the book reflects both the benefits and the limitations of that approach.

This book is not for those who are allergic to highly formalized arguments or to game theory. But for those who find this kind of analysis useful or are skeptical but willing to invest the effort in understanding, this slim volume will be a worthwhile addition to their libraries. The models are explained clearly, and the assumptions and conclusions are reviewed in the text.

After a useful introductory overview of strategic stability issues and of the book by Kugler and Zagare, Michael D. Intriligator and Dagobert L. Brito explore the stability of mutual deterrence. In the book's most interesting chapter, they argue that mutual deterrence is now extremely stable. This is due chiefly to the large number of nuclear weapons possessed by the superpowers, which renders the power that strikes first vulnerable to a devastating "second strike" by the other. The nuclear arsenals are so large that mutual assured destruction (MAD) is secure for the foreseeable future even in the face of technological change. "Arms control" that seeks only to reduce the stockpiles of nuclear weapons or, worse, to eliminate them is destabilizing and highly dangerous.

A better focus of concern, they argue, is the danger of accidental launch, particularly if either superpower has a "launch-on-warning" posture that could begin a nuclear exchange based on false information. Other legitimate arms control concerns include preserving the system of mutual deterrence (with limits both on antisubmarine warfare and on a totally effective missile defense and with the construction of more systems that could reliably survive a first strike) and preventing nuclear weapons from falling into the hands of unstable leaders. (An additional concern would seem to be the coming to power of unstable or unwise leaders in a country already possessing nuclear weapons). They conclude that other arms control mistakes include nuclear freezes, "no-first-use" agreements, and the agitation of public hysteria about these issues.

In "Assessing Stable Deterrence," Jacek Kugler takes a more pessimistic view of strategic stability, arguing that wars are frequently initiated even when antagonists are of equal power and a leader could expect to pay great costs for doing so. This suggests the importance of exploring the risk-taking propensity of leaders as a way of predicting behavior. This is vitally important, but I have reservations about how it is built into the model. In fact, a very low probability of destruction may be sufficient to deter nonlunatics, in view of the catastrophe that the loss of a single city would be. The problem is in determining what level of expected destruction would be so unacceptable as to deter a particular leader (or set of leaders) from initiating a nuclear exchange or, more broadly, from taking any actions that could lead to one. It is most difficult to do this in the abstract, as game theorists know best.

The editors argue in "Risk, Deterrence, and War" that MAD is not a stable basis for deterrence. Even in the face of expected catastrophic losses, risk-acceptant decision makers can destabilize a situation by provoking a nuclear confrontation. Ironically, although risk-acceptant leaders are the most dangerous, risk-averse leaders may also be a threat if they tempt an opponent who knows of this propensity to provoke a nuclear challenge. Risk-neutral leaders provide the most stability.

In "A Darwinian View of Deterrence," Christopher H. Achen argues that certain nonrational factors favored by evolution, such as aggression and willingness to retaliate, powerfully affect deterrence. A "Darwinian" perspective can help to explain outcomes that would not be predicted from rational choice models alone.

Steven J. Brams and D. Marc Kilgour analyze a deterrence game based on chicken in "The Path to Stable Deterrence." The key difference is the addition of a credible threat of retaliation in response to preemption. Although retaliation may not be "rational" in the short run, the expectation of some probability of its occurrence may move the equilibrium of the game from preemption to deterrence. In such a situation, the expectation of a "nonrational" reaction would actually increase strategic stability.

In "A Stability Analysis of the US-USSR Strategic Relationship," Frank Zagare attempts to account for the lack of superpower war since 1945. Most interestingly, "the capability requirements for successful deterrence are much lower than are generally supposed" (p. 147), at least so long as the credibility of retaliation is high. Stability is not assured either by an excess of missiles or by leaders so obviously concerned about the costs of nuclear war they thereby reduce the credibility of their nation's deterrence.

Formal modeling is absolutely dependent on the analyst's assumptions, in this case about the most important factors for strategic stability: decision makers' preferences and their probable actions in different situations. Unfortunately, it is precisely in this area that the least is known, and different sets of assumptions produce dramatically different conclusions with diametrically opposed policy implications. That some of the conclusions of the analyses are counterintuitive or contradictory should not be surprising. Real-world outcomes are also affected by factors outside the model, such as uncertainty, misinformation, distractions, mind-sets, and chance—and may even be determined by them. To their credit, the editors are aware of this and do not exaggerate the direct applicability of their results. Although final answers are elusive, those who are seeking ideas about how to increase strategic stability will find many suggestions here.

JOHN ALLEN WILLIAMS

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Irrationality in International Confrontation: By Robert Mandel. Contributions in Political Science no. 185. New York: Greenwood, 1987. 147p. $37.95.

Irrationality in leadership and decision making is clearly an important topic for study. Nevertheless, it has been a neglected one. In the selected bibliography that Mandel includes in his book there are references to psychological, decision-making, and related literatures, but there is little available directly on the phenomenon of irrationality. This book seeks to fill the gap.

The book includes only one chapter on the nature of irrationality. In this the author reveals a typology. Then, having explained his methodology, Mandel moves into a series of case studies that seek to demonstrate the different forms of rationality contained in his typology.

The typology is a simple one. There are three types of foreign policy decision making: low fluctuation, which is sluggish and stagnant; medium fluctuation, which is coherent and calculated; and high fluctuation, which is rash and reckless. The first and last of these are termed irrational. The second, medium fluctuation, is rational, being coherent and calculated.

In explanation of his conception, Mandel argues that irrationality can enter into every stage of decision making: identifying the problem, gathering information, arriving at a decision, implementing the action, and the feedback stage.

The sources of such irrationality vary. The author stresses human frailty, constraints on human reason, the bureaucratic environment, the roles that are being enacted within government structures, and the social and cultural contexts. There are also levels of information and time pressures to be taken into account.

It will be seen at once that Mandel is not dealing with madness. Indeed, he makes it clear at the outset that irrational behavior may be conducted by rational persons. His concept would have been still clearer if he had adopted the David Singer notion of *nonrational* rather than *irrational.*

This probably would have led him to an aspect he seems not to have taken into account—the way decisions are made to satisfy an immediate political gain, even though it is anticipated that there will be a longer-term negative outcome. This could possibly be the most frequent source of "irrational" behavior in political life.

The use of *nonrational* would also have helped in dealing with "perceived versus genuine irrationality" (p. 13). His interpretation of case studies gives the impression that in his view there is a large amount of "genuine irrationality" as defined by him, whereas a deeper analysis of the phenomenon could have led him to the view that the cases he was studying were cases of perceived irrationality, perceived madness, for instance, the perception of Iran by the Reagan administration.

Five chapters of this book are devoted to case studies and their evaluation. There is one final chapter on policy implications and prescriptions. There are those who attach importance to case studies as a methodology, but for me case studies are not a prime source of insights, merely examples to put some flesh on ideas derived from the theory within which the cases are interpreted. I would have liked to have seen more attention given to developing the theoretical aspects of irrationality. For example, almost no attention was given to irrationality as a perceived phenomenon, that is, as a means of explaining behaviors that the observer cannot understand, whether through lack of knowledge of, or lack of sympathy with, the reasons that give rise to the observed behavior. Our foreign enemies are mostly "irrational." This labeling makes unnecessary any further analysis.

Irrationality as a subject is part of decision-making theory, which in turn is part of system science and philosophy. Rational behavior implies commencing with a hypothesis, examining it in the light of all information available in any discipline, deducing an explanation or theory, and deducing from that an approach to the problem—which in turn suggests the application, from which there is feedback into the original hypothesis. Starting with a notion of rational behavior would have led to observations of how traditional political decision making focuses only on approach—usually ideological—and on action. If there is failure, it does not call for reexamination of any basic hypothesis of theory but is attributed to lack of sufficient action, and the remedy is more of the same. This truly traditional notion of what is "rational" would have demonstrated how nonrational most decision making is.

The "abductive" thinking of C. S. Peirce could be a basis (see D. H. Mellor's *Science, Belief, and Behavior* [1980]). Some such theoretical framework from which to deduce "irrational" behavior, making use of cases as examples only, would have led to a richer analysis and explanation of the phenomenon being exposed.

The book presents a challenge. Here is a topic that deserves more study.

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Book Reviews: International Relations

Arms Control and Nuclear Weapons: U.S. Policies and the National Interest. Edited by W. Gary Nichols and Milton Boykin. New York: Greenwood, 1987. 136p. $29.95.

This provocative slender volume offers readers a lively debate directed toward creating an informed public discussion that will center attention on what is clearly the critical issue of our times: how to survive the nuclear age.

In his introductory chapter, General George Seignious sets the pace by making it clear just what the stakes are. He cites a speech given by General Omar Bradley more than 30 years ago in which Bradley said, "If we are going to save ourselves from the instruments of our own intellect, we had soon better get ourselves under control and begin to make the world safe for living." Arms control is serious business. Among the lives we save will be our own.

Seignious stresses the fact that SALT I and SALT II have to be understood in their own terms, namely that the offensive systems of the United States and the Soviet Union were asymmetrical in makeup and in strength. The Anti-Ballistic Missile Treaty forced both nations into an acceptance of vulnerability and thus put into place the concept of mutually assured destruction of cities and populations, and that is still the case.

Kenneth Adelman, former director of the U.S. Arms Control and Disarmament Agency, discusses the Strategic Defense Initiative (SDI). He argues that SDI offers new opportunities to get out from under the balance of terror. He sees it as an option that just might be better than holding populations hostage to nuclear death while piling on new weapons. He makes the point that SDI is focused on a nonnuclear defense against *nuclear weapons* and argues that it would save the world from a "fail-safe" scenario under which a president might have to ride out the destruction of a major city while waiting to see if it were the result of a deliberate strike or rather a dreadful mistake or miscalculation. To the charge that SDI would be an immoral program, he comments that reducing the risk of war can hardly be against morality.

This point of view is directly challenged by Molly Ravenel, a former member of the National Governing Board of Common Cause. She believes that common sense should take us in another direction and that the SDI program would create uncertainty on the part of the Russians and tempt them into a preemptive strike. What President Reagan calls Mutually Secured Security is possibly just the reverse. Citizens should not be cowed into silence because they are not scientists. The concept of SDI is not incomprehensible, nor are its risks. The public should be alert to the loopholes in the language of strategy.

Boykin in his critique of U.S.-Soviet arms control negotiations argues that there are no sensible military uses for nuclear weapons, that they are political weapons and that Clausewitz' concept that war is but an extension of politics by other means has to be taken with some caution. War might be fatal to all. He cited the argument of Carl Sagan that war could be suicide in terms of the nuclear winter theory. Boykin makes the case for nuclear morality based upon the simple principle that life is good and should be preserved for all.

Ambassador Gartoff examines in his essay the nuclear weapons policy of the Soviets, going back to their attempts in 1940 to make an atomic bomb. He argues that contrary to the thinking of many U.S. citizens, the Russians were never very far behind and that that remains true today. Soviet policy has evolved, and arms control is not a thing in itself but rather an instrument of policy. He feels that SDI is destructive to this approach, since it forces the Soviets to emphasize offensive actions.

Jack Perry tries to get the reader to think beyond the simple East-West dialog. The superpowers are not the only powers. There are the U.S. allies, the Soviet allies, and the rest of the world. Their interests affect the nuclear challenge, and they raise the issue of just whose security is at risk.

Although the book was published in 1987, these essays were written in 1985—two years before Gorbachev came to Washington and the framing of the Intermediate Nuclear Forces agreement. Yet they all retain their relevancy. No longer can scientists be separated from citizens, nor can the concerns of the military ignore the moral messages of religious leaders. If there is a central message to this book, it is that we are all in this together. In the words of the late President Lyndon Johnson, "We must reason together to make things come out all right."

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Peace in Parts: Integration and Conflict in Regional Organization. By Joseph S. Nye, Jr. Lanham, MD: University Press of America, 1971. 1987 Reprint. 210p. $22.50 cloth, $9.75 paper.

During the 1960s and early 1970s, the study of regional integration occupied a prominent place in international relations theory. The European Economic Community had clearly transformed economic and political relations in the traditional heartland of world politics; and other regional communities were being established in both developed and developing areas for a wide variety of purposes. Beyond its unquestioned empirical importance, furthermore, regional integration was generally considered "good" from a normative perspective. Thus, regional communities are assumed to make an important contribution to the economic growth and well-being of their members and to the control and eradiction of political and military conflicts within the region.

Joseph Nye's *Peace in Parts* (1971) represented a major scholarly contribution to the intellectual debates about the nature and consequences of regional integration on several fronts. It presented perhaps the most incisive yet comprehensive model of how the integrative processes proceed and expand from narrower economic to broader political issues and institutions and also discussed the development of indexes of the key concepts in this theory with some imagination. Nye also made the felicitous distinction between "economic microregions" and "political macroregions" and showed with a good mix (or "integration") of theory and case study that they had somewhat different effects upon political and economic outcomes. Thematically, the book was organized around five major hypotheses about how regional integration could promote peace in the contemporary world. Nye concluded that while integration had little impact upon how people related to their state, several regional communities had proved significant for the conduct of interstate relations, thus making a modest contribution to the creation of "islands of peace" in a conflictual world. This analysis incidentally reflects another striking characteristic of Nye's work. Unlike the quite optimistic conclusions that dominated the study of integration, Nye's models indicated the "limits of integration" with what turned out to be admirable foresight.

Peace in Parts, therefore, is clearly an important and valuable work of scholarship. It reflected and synthesized a decade of theoretical and empirical work in what was then a central concern of international relations theorists, but it also looked ahead to the general stagnation and failure of regional communities. This decline of regionalism was multicausal. State sovereignty and nationalism proved much more intractable at both the elite and mass levels than scholars anticipated. The transportation and communications revolutions rapidly created a global economic interdependence that made most regional cooperative schemes obsolete. The world economic volatility of the 1970s and 1980s greatly increased competition for sharing the burdens of a contracting "pie." Regional political conflicts became increasingly entangled in superpower rivalry as both the United States and USSR tried to play "global policemen." As a result, regional communities, especially in the third world, lost most of their ability to promote economic growth and reduce political conflict, the Central American Common Market being perhaps the most miserable and spectacular failure, thereby sending international relations scholars scurrying to find more profitable paths of inquiry.

Given this decline and near demise of integration theory, one may certainly question the reissuance of *Peace in Parts*. In a brief introduction, Nye linked regional integration theory to the development of the "liberal"—as opposed to the "realist"—perspective on international affairs. Thus *Peace in Parts*, as one of the best representatives of the analysis of regional integration, can be viewed as having played a significant role in the intellectual history of an important strand of international relations theory that went on to analyze global interdependence and international economic regimes and to ask what will happen to the global political economy "after hegemony."

In fact, the declining hegemony of the

United States in the international economy and the seeming willingness of the two superpowers to curb their global competition suggest that regionalism may again become relevant but in a somewhat different way from that visualized by integration theorists of 15 years ago. In the economic sphere several regions face similar problems in the global economy (e.g., East Asia's need to combat rising Western protectionism, the debt crisis in Latin America, political reform and technological upgrading in the Soviet bloc, responding to the industrial competitiveness of newly industrialized countries in Western Europe and North America, and so forth). Thus, the coming decade may witness a trend toward regional negotiations over international economic regimes rather than the revitalization of common markets. Politically, growing similarity of economic interests may make regional members more sensitive to the costs of conflicts and to the fact that superpower interventions usually exacerbate rather than solve crises. Thus there is at least some chance that primarily regional negotiations, whether they involve formal bodies or not, may again work along the "modest" lines depicted by Nye's analysis as the international system increasingly moves into a phase of the "deconcentration" of political and economic power.

CAL CLARK

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The Foreign Relations of North Korea: New Perspectives. Edited by Jae-kyu Park, Byung-chul Koh, and Tae-hwan Kwak. Boulder, CO: Westview, 1987. 491p. $39.50.

How is foreign policy made in North Korea? How have North Korea's foreign relations evolved in the past 40 years? What are military and economic capabilities and constraints in the conduct of foreign policy in North Korea? And how can we best characterize North Korea's contemporary diplomatic relations with other countries, including the U.S. and the Soviet Union? These are some of the questions dealt with in this collection of 17 articles by 16 authors. The articles vary in style, approach, and emphasis; but all the authors appear to have attempted to highlight their research findings based on a variety of most up-to-date, and often hard-to-find, source materials. This volume offers an excellent insight into the study of foreign relations of North Korea, one of the most secretive countries in the world today.

The book, designed to provide a "theoretical analysis," is divided into five parts: an overview of North Korea's foreign policy making; "domestic sources" of foreign policy; and North Korea's relations with Communist countries, Western Europe and Japan, and South Korea and the rest of the world. In part 1, Jae-kyu Park outlines the "fundamental premise" and goals of North Korea's foreign policy. Unification is considered as the "ultimate goal." In Byung-joon Ahn's view, however, such consistent goals have accompanied changing policies and strategies in response to changing internal and external situations. Byung-chul Koh, in an attempt to explain the foreign policy-making process, devotes much of his paper to analyzing three factors he developed: operational environment, psychological environment, and the structure and routines of decision making.

Part 2 consists of articles on ideological, military, and economic capabilities and constraints relevant to North Korea's conduct of foreign policy. Han S. Park offers a view that Kim Il-sung's doctrine of *juche* (self-reliance) will become a source of constraints in North Korea's foreign policy implementation since it would not afford flexibility in adopting the rigidly controlled regime to changing situations. The article by Jong-chun Baek presents useful data on North Korea's military strengths, compared with South Korea. The three "basic" military capabilities North Korea has developed over the years are its capabilities as a revolutionary base, revolutionary capabilities in South Korea, and international revolutionary capabilities. And Joseph Chung deals with North Korea's record of economic development, focusing on the various economic plans and pointing out specific reasons for the relatively poor economic performance record in comparison with South Korea in recent years.

Part 3 is an analysis of North Korea's relations with the Soviet Union, People's Republic of China, and Eastern European countries. Joseph Ha and Linda Jensen contend that a unified Korea under a Communist regime, even under Kim Il-sung, is not what Moscow really wants. Chin-wee Chung reviews North Korea's foreign policy in the context of the Sino-Soviet dispute and concludes that Pyungyang's relations with Beijing—compared to relations with Moscow—are likely to remain friendly because of historical and cultural similarities between the two. North Korea's relations with Eastern European countries have been on good terms since 1948, according to In-young Chung, who also highlights policy implications of Kim Il-sung's recent visits to the Soviet satellites.

Parts 4 and 5 deal with North Korea's diplomatic relations with the United States (by Ralph N. Clough), Japan (Jung-hyun Shin), Western Europe (Gregory F. T. Winn), South Korea (Tae-hwan Kwak), Southeast Asia (Kook-chin Kim), Middle East (Chung-in Moon), Latin America (Man-woo Lee), and Africa (Jae-kyu Park). According to Clough, North Korea has failed to obtain what it wanted from the United States, due to its "harsh tactics" and underdeveloped diplomacy. Shin does not see any drastic change in North Korea's hostile relations with Japan in the foreseeable future although both countries might try to improve relations. In spite of recent commercial agreements, Winn concludes that prospects for North Korea's relations with Western Europe remain not very encouraging. Kwak, after identifying fundamental differences between North Korea and South Korea in their unification policies, offers a "cautiously optimistic" view on the future of the inter-Korean dialog. Southeast Asia has been a crucial diplomatic target for North Korea, according to Kim, but in recent years North Korea has lost its edge to South Korea, particularly among Alliance of Southeast Asian Nations. Moon's excellent study shows that North Korea's influence over some parts of the Middle East is also on the wane. However, as Lee and Park suggest, North Korea, compared with South Korea, has done a rather impressive job in establishing diplomatic relations in Latin America and Africa.

This volume is a collection of research conducted mostly, if not exclusively, from South Korea's vantage point, with ample room for further debate on the objectivity of the research findings and interpretations. While the organization of the topics is excellent, readers of the book will find several articles with virtually the same or similar historical or chronological accounts of events. On balance, however, this book covers a wide range of topics relevant to North Korea's foreign relations, and the editors attempt to cover the Communist state's contacts with virtually all the regions. The editors are to be commended for their efforts. Readers of this volume will find the bibliography section particularly useful in the study of North Korea's foreign relations. The volume was not designed to be a textbook; it may be used as a reader in international relations or comparative government courses for both undergraduate and graduate students. This timely book is certainly a welcome addition to the much-needed literature on the communist regime on the Korean peninsula.

KEON-S. CHI

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Turkey: America's Forgotten Ally. By Dankwart A. Rustow. New York: Council on Foreign Relations, 1987. 155p. $14.95.

Because of the rapid transformation Turkish society has undergone during recent years, books that would be current in more gradually changing contexts become outdated when the object of study is Turkey. If one were to read a book on Turkish politics written five years ago, for example, one would have little idea what the Turkish political party system looks like today. Similarly, in terms of foreign policy one would find it difficult to believe that a country described five years ago as underdeveloped, unstable, and authoritarian felt developed, stable, and democratic enough to apply for full membership to the European Community in 1987. Rustow's book is a timely publication, coming at a moment when there is a notable paucity of writing on contemporary Turkish society, politics, and economics.

Benefiting from his extensive familiarity with Turkey, Rustow has adopted a historical perspective. In this way, the description of today is rendered more comprehensible by relating it to past developments. The book may best be described as a brief study of socio-economic and political change in Turkey. As a study of change, it may be of interest to students of change in other environments.

The book is brief, and it appears to have been written not for consumption in the academic community but for readers interested in Turkey for reasons (such as business), who would like to get a summary description of social, economic, and political life in Turkey. This approach has its advantages: the book is easy reading, and it draws a comprehensive but elegant picture of change in a country. It does bring with it, however, a number of problems. Because a theoretical concern is lacking, the relevance of the work to specialists who do their research in other parts of the world is severely curtailed. Similarly, because the analysis is brief, the reader may be left wanting to know more about a number of assertions or generalizations offered in the book. A case in point is the explanation that the tradition of secularism in Turkey and the differences of religious sect between Iran and Turkey make it most unlikely that an Iranian-type fundamentalism may carry the day in Turkey. This is a judgment that I personally share, but many readers might want to know in greater detail the nature of Turkish secularism and the particular version of Sunni Islam professed in Turkey and how it differs from Iranian Shiism in its ideology, practice, and organization, before they accept Rustow's analysis.

The book is organized around five major themes, which also constitute five of the six chapters in the book. The first chapter is an introduction. In the subsequent five chapters the westernization of Turkey, the change in Turkish economic policies, the nature of Turkish democracy and its evolution, Turkish-U.S. relations, and the major features of Turkish foreign policy are examined. There is, at the end of the book, a list of other sources the interested reader might consult on the topics taken up in each chapter. This list is short and not particularly comprehensive.

We have here an interesting book for the lay reader, and the student of socioeconomic and political change. The expert on Turkey may find the book too general, but those interested in the study of change may discover here a case they would like to learn more about.

ILTER TURAN

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The UN Commission on Human Rights. By Howard Tolley, Jr. Boulder: Westview, 1987. 300p. $29.95.

Howard Tolley, Jr. of the University of Cincinnati has written an interesting and useful monograph describing the history of the United Nations Commission on Human Rights from its founding in 1946 until 1986. He adds an afterword on the commission's 1987 activities. He also asks two important questions. First, can a policy-making institution responsible for drafting international norms be successful at enforcing those norms? And second, can a policy-making institution composed of governments provide impartial and effective protection to citizens of those governments? The answer to both questions, of course, is *no*, and Tolley documents the answer in appropriate detail.

He begins by describing some of the forerunners of the commission, including such conceptual ancestors as the U.S. Bill of Rights, the nineteenth century Geneva conventions, and the Mandates Commission of the League of Nations. From these historical antecedents and the World War II holocaust-inspired vision that something had to be done to protect human rights came the institution of the UN Commission on Human Rights, founded in 1946 under the aegis of the United Nations Economic and Social Council.

The commission's first great success was the Universal Declaration of Human Rights, approved soon after its founding. This declaration set a standard and tone that the commission subsequently found difficult to live up to, as it was forbidden by its parent council to take any enforcement action against complaints of human rights violations. After all, the UN itself was forbidden by its charter to interfere in the domestic affairs of any of its members, and the prevailing view in the early days of the organization was that a government's treatment of its own citizens was essentially a domestic matter and not a threat to the peace. So the early years of the commission were spent principally in research, in the setting of standards of governmental conduct, and in promoting the idea of human rights.

After the commission moved into the 1960s, its membership eventually increased from 18 to its present 43, and control shifted from the hands of a Western-oriented majority to a third-world majority with a much different agenda. Resolutions and activities of the commission were politicized by the East-West and the North-South conflicts, and the focus moved to anticolonialism and antiracism, largely in South Africa and Israel.

In 13 years of debate, Tolley writes, only three regimes—South Africa, Israel, and Chile—were actually singled out for special concern. Tolley describes well the double standard used by the commission in the 1960s and 1970s, as third-world countries refused to denounce the human rights violations of their own governments, and the so-called Western values of freedom of speech, press, religion, and assembly were given low priority.

But the author concludes on a more optimistic note, suggesting that a better balance now exists in the commission and that a "tenuous balance . . . has created a fragile vitality which survived the deadening rhetoric" (p. 219). Tolley adds, "Equally important, the Commission has cracked the citadel of national sovereignty first by drafting standards and then by eliciting government responses to complaints about their misconduct" (p. 220).

Clearly, the UN Commission on Human Rights serves in many ways as a microcosm of the United Nations itself. Like the latter, the commission was formed to correct the perceived causes of World War II, controlled in its early years by a pro-Western majority, dominated in more recent years by a third-world majority intent on a much-different agenda, politicized by conflicts that had little to do with the main purposes of the institution, and plagued by institutional and procedural problems which have tended to reduce its value to informed publics.

Tolley's book is a valuable addition to the literature on the United Nations and its subsidiary organs and stands alone as a current history of the Commission on Human Rights. If the book's style is not exciting, the story it tells is, and the conclusions are balanced and carefully drawn. The documentation, bibliography, and index are excellent. This monograph will be important reading in the next few years for scholars who are working in the field of international organization or human rights.

THOMAS A. SARGENT

Ball State University

From Brezhnev to Gorbachev: Domestic Affairs and Foreign Policy. Edited by Hans-Joachim Veen. New York: St. Martin's, 1987. 378p. $45.00.

The volume under consideration is a revised edition of the proceedings of a joint German-U.S. conference on post-Brezhnev Soviet domestic politics and foreign policy held in 1984. It is broken into three sections, dealing with Soviet economy, culture and society, and the political arena. The purpose of the book is twofold: to enhance German-U.S. cooperation in the field and to deepen our understanding of the nature of, and prospects for, political change in the USSR.

It has several strengths. Most significant is the role it plays in bringing to U.S. attention the depth and breadth of West German scholarship on the Soviet Union. The offerings by Karl-Eugen Wadekin on agriculture, Jochen Bethkenhagen on energy, Klaus Schroder on debt, and Christian Schmidt-Hauer on leadership transition, among others, reflect a level of dispassionate and careful scholarship all too often absent from debate on the USSR in this country.

Although in this sense, the work makes an important contribution, it is flawed in a number of ways. First, there is no sustained effort to draw together the various strands of the analysis into a synthetic whole. A lot of ground is covered without ever really getting anywhere. By the end of the section on energy, for example, one has read that the "energy situation offers very favorable prospects for Soviet leaders" (p. 53) and that "energy will present much more in the way of problems, much more in the way of investment demands, much more in the way of technological requirements that will burden the economy than it will present in terms of opportunities for economic growth and expansion of trade" (p. 57). One expects differences of interpretation in a volume of this type, but such fundamental disagreement is disorienting to the nonspecialist.

Moreover, there does not appear to have been any real attempt at quality control. Although some of the contributions are well-documented and amply developed, others are brief, undocumented, and superficial. Perhaps more seriously, in some cases constructive scholarship gives way to crass politically motivated distortion. Richard Staar's incomplete and inaccurate characterization of the activities of the Pugwash movement, for example, is insufficient to justify his dismissal of it as a "sounding board for anti-American and pro-Soviet propaganda" (p. 240). He leaves the impression that anyone believing that dialogue with the USSR may be useful is a dupe. His further remark, with regard to the U.S. campus protest movement of the 1960s, that "the meaning of words like peace or academic freedom and the 'people' in reality signified coercion and the will of a small minority that used any available means to introduce anarchy" suggests that he is more concerned to vent his spleen than to engage in serious discussion of Soviet politics (p. 250).

Finally, in many instances analysis stops in 1984, the date of the conference on which the volume is based. Given the character of the intervening three years, systematic efforts to update research results would have been prudent. It is odd in a work published in 1987 to run into an analysis of the process of economic reform that makes no mention whatever of the Gorbachev era. Although some of the more substantial contributions dealing with the economic structure and with the political sociology of the USSR (e.g., Meissner's contribution on social change and its impact on the social structure of the party) have lasting value, much of the work is dated and of little utility to the student of current politics in the USSR.

The work shows this weakness with a number of earlier collections, such as Bialer's *Domestic Context of Soviet Foreign Policy* (1981) or Byrnes' *After Brezhnev* (1983). All suffer by comparison to the most recent edition of Colton's *Dilemma of Reform in the Soviet Union* (1984), which is as close as one currently gets to a definitive treatment of the subject.

S. NEIL MACFARLANE

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Security in the Middle East: Regional Change and Great Power Strategies. Edited by *Samuel F. Wells, Jr., and Mark A. Bruzonsky.* Boulder: Westview, 1987. 366p. $38.50 cloth, $15.95 paper.

The Middle East in Global Strategy. Edited by Aurel Braun. Boulder: Westview, 1987. 274p. $31.50.

The Persian Gulf and the West: The Dilemmas of Security. By Charles A. Kupchan. Boston: Allen & Unwin, 1987. 254p. $39.95 cloth, $14.95 paper.

It is clear that at a time when even such practitioners as former secretary of defense Caspar Weinberger (speech to the Comstock Club, Sacramento, 19 July 1985) reveal a considerable measure of sophistication in relating the efficacy of resort to military force to the prevailing political and social contexts, scholarly works on strategy may not remain parochial. In fact all three books under review are aware that strategy in the Middle East cannot be fruitfully analyzed in terms of its military dimension alone and consequently devote most of their contributions to a consideration of the relevant political and social contexts.

In view of the fairly large volume of scholarly output currently available on different aspects of the subject, the more recent publications should be subject to higher standards than in the past, when little was known. I have used three primary criteria to gauge the quality of each contribution. The first criterion consists of theoretical sophistication, that is, the degree to which the authors are aware of their own theoretical frameworks and operative assumptions and ultimately make a contribution to the refinement and further advancement of the available theory. The second consists of methodological rigor and objectivity. This is particularly significant in the case of contributions on the Middle East, which must inevitably deal with such conflicts as those between the United States and the Soviet Union, the Arabs and Israel, Iran and Iraq, and nationalism and religion, each of which has the potential to evoke a considerable amount of partisanship and advocacy even under the rubric of social science. The third consists of the level of contribution to policy-making and implementation, that is, the degree to which the authors identify security objectives, relate such objectives to other objectives that are engaged and to the available means, generate well-conceived options in terms of cost-benefit assessment and trends in the situational contexts, and so on.

Security in The Middle East: Regional Change and Great Power Strategies consists of 16 papers, most of which were originally presented to a seminar held at the Woodrow Wilson Center for International Scholars from 1981 to 1983 and subsequently modified in light of fast-moving events in the Middle East, particularly developments in Lebanon. Despite variation in the quality of the chapters, the collection as a whole rates rather high in terms of objectivity and contribution to policy-making. It is weaker in the area of theory. In fact, theory is not a strong suit in any of the three books.

In the introduction, Samuel Wells, Jr. and Mark Bruzonsky provide a fairly comprehensive outline of the questions related to internal dynamics of the regional states and their relations with the West. While such issues as the role of religion in the area and internal political dynamics of the United States itself may have received more attention, the editors have made a deliberate, and to a great extent successful, effort to overcome parochialism and set a tone of objectivity that has paid dividends in the overall quality of the volume.

Part 1, "Stability and Change within the Region," is composed of seven quite competent but at times somewhat thin chapters on Syria by John F. Devlin, Lebanon by Augustus Richard Norton, Israel by Bernard Reich, Egypt by Ali Hillal Dessouki, Saudi Arabia by Adeed Dawisha, Iraq by Christine Moss Helms, and Iran by Robert Litwak. Since the authors are as a rule political scientists, they are subject to higher theoretical standards than other contributors—historians, lawyers, journalists, and government officials. Consequently, one could have expected greater analysis, less description, and more concern with clarification of such concepts as stability, which has tended to be confused with the status quo. This tendency has in turn led to prescriptions for failure, because in the long run change cannot be stopped altogether, even by mightier forces than those at the disposal of the United States.

Part 2 deals with the Palestinian problem. It consists of three thoughtful essays that make serious contributions to problem solving and collectively constitute the most distinguished part of this volume: Herbert Kelman's "Creating the Conditions for Israeli-Palestinian Negotiations," Simha Flapan's "The Long Search for Peace: A Progressive Israeli View," and Bruce Kuniholm's "The Palestinian Problem and U.S. Policy." Kelman brings in a psychological dimension that is badly needed but usually missing in treatments of U.S. relations with the Middle East. Unfortunately, there is no Palestinian or Arab scholar represented in this section. Given this fact and the prevailing biases of U.S. policies in the last four decades, which, intended or not, have substantially contributed to the plight of the Palestinians, one may conclude that there is nothing new here. But there is something new. Flapan, a former director of the Arab department of Mapam, bends over backward to present the other sides' arguments as well as his own before arriving at his conclusions. In fact, concern for objectivity and the nature of operating assumptions also characterize the essays by Kelman and Kuniholm. What is missing in this part is a separate chapter of equal objectivity on Soviet policies toward the Palestinians and the potential Soviet role in achieving peace.

Part 3 consists of five essays of uneven quality: "Middle East Oil and Industrial Democracies" by Robert Lieber, "Soviet Decisionmaking for the Middle East" by Dennis Ross, "Soviet Policy in the Middle East" by Shahram Chubin, "American Diplomacy and Arab-Israeli-Palestinian Peace since 1967" by Harold Saunders, and "A Political/Military Strategy for the Persian Gulf and Southwest Asia" by Kuniholm. Although each chapter has something useful to say, insufficient concern with theory and methodological rigor has taken its toll. Lieber, for example, takes issue with the West European view that settling the Palestinian conflict would improve the Western position, asserting that "such a peace could unleash even more divisive tendencies than currently exist. In this regard the observation of Fouad Ajami is worth quoting at length. He judges that a major Middle East peace initiative by the United States would greatly inflame the passions of the extremists" (p. 230). Yet the quote that follows does not sustain such an assertion (p. 231). Instead, it refers to other sources of "terror" as well. But surely this is not a major discovery, and referring to other sources of terror does not make the impasse on the Palestinian issue irrelevant or peace a greater source of extremism and passion, unless sustainable evidence and analysis to this effect are presented, which they are not. To cite another example, Chubin states, "How far Iran will be permitted to evolve in its own way and at its own pace will be an indicator of Soviet intentions." Why would this be an indicator of Soviet intentions but not of Soviet capabilities? As the literature of decision making clearly indicates, objectives can be meaningfully considered only in terms of the relevant available means and cost-benefit assessment. It would not have been helpful to the Afghan Mujahedin, for instance, to be persuaded by such arguments as those of Chubin because there is little doubt about the nature of the Soviet intentions in their case. But it would have been helpful if this essay had taken the evidence from Afghanistan—as well as that from Vietnam and Lebanon—to examine its operative assumptions. Furthermore, it should be noted that the Soviets are unable to achieve their objectives in Afghanistan despite the existence of a friendly and obliging government, something which does not exist in Iran. In any case, the Iranian leaders do not seem to be as concerned as Chubin about Soviet permission when it comes to their initiating and implementing of policies. On the whole, a little more social science and less journalism would have helped most of these substantive essays. Kuniholm, clearly well-versed in policy analysis, verifies this proposition by producing the outstanding essay of this part. It demonstrates the ability of historians and area specialists to produce rigorous contributions that are of utility to both social scientists and decision makers.

The Middle East in Global Strategy begins with an introduction by the editor, Aurel Braun, which refers to the works of Clausewitz and André Beaufré but does not refer to more recent developments in strategic thinking and decision-making theory. Despite the stated fondness for Beaufré, the essays dealing with Israel, the Arabs, and the United States, particularly those in parts 1 and 2, do not as a rule provide an objective assessment of "political, economic, diplomatic, psychological, and military dimensions" (p. 6). They accord somewhat more readily to the statement that "strategy for Beaufré is an art rather than a science because it cannot have clearly determined rules" (p. 7).

The problem is not so much that these contributions to illuminating conditions of the Arab states and Iran and assessing the United States' national interest include Israelis—with teaching background in Haifa, Hebrew, Bar-Ilan and Ben-Gurion universities—and some committed pro-Israeli Canadian and U.S. scholars but no Arab or Iranian authors. Rather, the problem stems from the effect of shifting paradigms and standards, exacerbated by the absence of the countervailing influence of even such Israeli scholars as Flapan. Meir Zamir's "The Emergence of Syria?" for example, is quite correct in relating foreign and domestic policies and pointing out the problems posed for that country's external capabilities by its potential domestic instability. But the problems posed for the legitimacy, hence stability, of the pro-Western Arab regimes caused by the behavior of Israel and policies of the United States, undertaken in response to the pressures of domestic pro-Israeli groups, have received little attention. Yet it should be clear that if these regimes fall before the onslaught of nationalism, religion, or both, which have also been neglected or underemphasized despite their significance, the United States is likely to lose leverage in the Middle East; and when the United States loses, Israel probably does too.

In the same vein, Gerald Steinberg and Steven Spiegel's "Israel and the Security of the West" is a competent statement of Israel's advantages but not a serious discussion of its disadvantages for the Western countries. The most interesting case of shifting standards is represented in "Strategy and Politics: A U.S. Perception" by Howard Teicher, senior director for political-military affairs in the National Security Council. He argues that "Americans, as a moral people, want U.S. foreign policy to reflect the values that the nation espouses. But Americans, as a pragmatic people, also want an effective policy." Foreign policy is not made by the U.S. people, however, but by a few (mainly the president and his advisers and members of Congress, mostly the Senate) who make policies in the name of the people and not always according to the latter's values or benefit.

In reference to Reagan's initiative of 1 September 1982, Teicher maintains that the U.S. positions are (1) idealistic and moral, (2) pragmatic, and (3) realistic. Since these concepts are not systematically related to each other, they provide a perfect pretext for justifying one's own positions and condemning the alternative positions by shifting from standard to standard. When some people point out the immorality or illegality of our position, we will accuse them of not being realistic; and when we are not realistic in the pursuit of agreed-upon criteria of the national interest, we argue that we are moral. At the same time, we can use the opposite attributions for Arab or Soviet policies with which we disagree. When they are realistic, we brand them as immoral; when they are moral, we brand them as unrealistic, and so on.

It may be helpful to refer to some actual policies in this connection. The United States is supposed to adhere to a policy of nuclear non proliferation. When assistance funds to a country such as Pakistan—which supports our policies toward Afghanistan—are considered, Congress indicates great concern with nonproliferation policy, threatening to cut, or actually cutting, funds because of Pakistani potential in this area. But it has shown no interest in ever discussing the issue of a full-fledged Israeli nuclear arsenal and has contributed to its construction by substantially adding to the already generous funds usually requested for Israel by the executive. Many of the estimated 850–900 million Muslims perceive this as yet another sign of U.S. hypocrisy, as well as of a plan to make 3.5 million Israeli Jews dominant not only over 120 million Arabs but also over the entire Muslim world. Furthermore, eventually the Arabs and Muslims are also likely to acquire nuclear weapons and missiles, and it is not clear that such development would benefit anyone, especially Israel. When the U.S. government deals with the Soviet Union, it emphasizes human rights; when it deals with the Palestinians in the occupied territories it emphasizes that Israel's security, even over the annexed areas, overrides the right of the dispossessed. Another case is Reagan's initial support for Israel's right to all of Jerusalem and disagreement with Carter's position that Israel's settlements in the West Bank were illegal, hence strengthening the hands of those is Israel who have been opposed to some of the Camp David provisions. I do not know whether these behavioral patterns by Congress and the president would be regarded as "moral," "realistic," or both by Teicher. But the possibility that they may be neither certainly deserves some consideration, particularly in light of what our policy of simultaneously guaranteeing Israel's military superiority over its Arab neighbors and insisting that the parties to the conflict must find a mutually acceptable solution has produced for the Palestinians.

The sections on external influences consists of three essays on superpowers and three on alliances. Sanford Lakoff's "Power and Limits: U.S. Strategic Doctrine in the Middle East," provides a useful historical summary and raises meaningful questions about the limits and potential utility of resort to force. S. N. MacFarlane's "The Middle East in Soviet Strategy" is a fairly sophisticated examination of the elements of Soviet foreign policy and of the potential Soviet role in the resolution of the Arab-Israeli conflict. Edwin Fedder's "NATO and the Middle East" is the most distinguished of the essays in this part because of its objectivity, its ability to overcome parochialism, and its exposition of the sources of divergence between European and indeed even of Arab states' policies and those of the U.S. The other essays in this part consist of "The Superpowers and the Middle East: The Maritime Dimension" by Joel Sokolsky, "Alliance Politics in the Middle East: A Security Dilemma Perspective" by Avner Yaniv, and "The Warsaw Pact and the Middle East" by Braun.

Part 4 deals with the gulf. Roger Savory's "Geopolitical Impact of the Islamic Revolution in Iran on the Gulf Region" is not restrained in its castigation of either the Islamic Republic or its major perceived cause for success, Carter's administration. The "vital interests" of the United States in the gulf region, "which are now geopolitical rather than economic," consist of four items of which the first is "support for Israel." As to the resolution of the problems that this may cause for another admitted interest, "the security of the 'friendly' Arab states in the area," he readily provides it by ignoring the problems. This is followed by Litwak's "The Soviet Union and the Gulf Area." Zalmay Khalilzad's "U.S. Strategic Concerns: Deterrence Dilemmas in the Gulf Region" is the last and probably the best essay in this collection. He is quite aware that "concepts and plausibilities" are no substitute for "empirical evidence" and displays an understanding of recent developments in strategic theory, particularly of the writings of Thomas Schelling, Robert Jervis, and Alexander George. It would have been of considerable help to the general quality of this book if these developments had been incorporated in the introduction and guided the relevant contributions. Khalilzad also presents a provocative application of "the balance of interest" to the gulf and distinguishes the northern tier from Saudi Arabia and smaller oil-producing states in terms of the strategic requirements for mutually assured destruction (MAD). I, however, find it more fruitful and in touch with the empirical world to rely primarily on the Afghans and Iranians, perhaps with assistance from the West, to frustrate any Soviet conventional thrust into their lands, rather than delving headlong into speculative consideration of the incredible world of MAD in the context of the gulf.

Being a single-author book, Kupchan's *The Persian Gulf and the West* is considerably more integrated and focused than the other two volumes. Its basic purpose is to use the Persian Gulf as one particular area of the periphery "as a means of delving into debate about the Western security" and developing policy prescriptions. The introduction stakes out three basic security dilemmas for the United States in the gulf arising from "strategy vs. capability, globalism vs. regionalism, and unilateralism vs. collectivism" (p. 4).

Chapters 2-5 are primarily historical: "The Setting, 1946-73," "The October War and Its Aftermath," "Iran, Afghanistan and the Evolution of the Carter Doctrine," and "Rapid Deployment Force: Planning, Strategy, Operational Requirements." They are generally quite competent and sufficiently analytical to make them interesting to political scientists. Chapter 6, "U.S. Power in the Gulf: Military Strategy and Regional Politics," breaks some new ground. It addresses the difficulty of reconciling the requirements of different U.S. objectives, points out the "gap between strategy and political feasibility" stemming from some "key political assumptions," and concludes that the "task of linking military strategy to political reality . . . was less affected by bureaucratic dynamics and electoral concerns than was the process of policy formulation itself" (pp. 157–8).

Chapters 7 and 8 deal with relations between the United States and NATO. Both are well informed and analytical. The political problems, which constituted the main reason for hindering "the ability of the alliance to address security issues in the Middle East between 1950 and 1980," still remain important for the sustainable but somewhat overdrawn conclusion that "NATO as presently constituted is unprepared and unable to address adequately regional threats to Western security" (p. 208). An additional factor, which has not received adequate attention, is the seemingly long-term structural trends in the United States' comparative capability in the economic realm. If not halted, it will require difficult decisions in terms of priorities and allocation of resources. Whether such decisions can be made sooner—rather than later and at greater costs—is highly debatable, given the internal political system of the United States. But one thing is clear. Western Europe will have to assume a greater role—and, concomitantly, a greater voice—in determining and providing for its own security. It is in this light that chapter 9, "The Dilemmas of Persian Gulf Security," becomes particularly interesting and useful to both social scientists and policymakers.

Each of the three books reviewed makes some useful contribution to the subject of Western security. Future works may find it helpful to be more concerned with the adverse effects of reification of states and place greater emphasis on internal political dynamics not only of the regional states but also of the United States and Israel. Further, it may be worthwhile to place greater emphasis on long-term structural trends and their implications. This is particularly important for guarding against intellectual lag in coming to grips with the implications of change and against using frames of reference that are dysfunctional. Finally, analysts may find it rewarding to go out of their way to seek the views of those with whom they disagree, because, as Jervis noted long ago, "scholars and decision-makers are apt to err by being too wedded to the established view and too closed to new information, as opposed to being too willing to alter their theories."

G. HOSSEIN RAZI

University of Houston

The Manner of Giving: Strategic Aid and Japanese Foreign Policy. By Dennis T. Yasutomo. Lexington, MA: D. C. Heath, 1986. 147p. $22.00.

Japan stands figuratively as a "nexus" in international society, the place where East-West and North-South relations come together. In the coming years—indeed, through the next century—Japan will bear an incalculably heavy responsibility encompassing not only politics and economics but also all aspects of human existence. Today, Japan is the number two nation in the amount of foreign aid it provides to third world nations, and foreign aid is one of her major foreign policies. This book makes a significant contribution to the literature about Japan's current international roles and foreign aid activities.

Foreign aid is the expression most frequently used to describe the flow of financial and technical resources from the developed world to the underdeveloped world. The understanding of foreign aid processes, problems, and policies is difficult even for the most sophisticated observer because there are so many facets to economic development, so many conflicting viewpoints. The whole foreign aid complex is intricately enmeshed in the fabric of national interests and foreign policies. The author untangles Japanese foreign aid policy, providing insights into its motivations and effects.

The author's central message is stated in the five sets of questions presented at the beginning of the book (p. 6). What role does domestic politics play in shaping strategic uses of aid? What is the exact relationship between economic assistance and defense policy? What effect will globalization have on Japan's traditionally regional aid focus (the association of southeast asian nations [ASEAN])? Would Japan have strategic aid policy without outside prodding? Exactly what role does strategic aid play in Japanese policy and what impact is it likely to have in the future? By answering these questions, the author attempts to chart Japanese foreign policy through the 1980s and beyond.

The highlight of the book is chapter 5, entitled "The Limits of Strategic Aid: Globalism versus Regionalism." The chapter offers a unique analysis of eight cases—in Latin America, Africa, the Middle East, the ASEAN nations, China, Pakistan, and Indochina—to illustrate five interrelated objectives: economic well-being, national prestige, domestic support, peace diplomacy, and national security. The author concludes that "strategic aid has survived in a skeptical domestic environment because of conceptual ambiguity. Comprehensive national security, countries bordering areas of conflict, areas which are important to the maintenance of peace and stability of the world, interdependence, humanitarianism—these concepts are strategic aid's relatives, but none can claim parentage" (p. 114). Foreign aid is not entirely a one-way street since the recipient countries themselves contribute significantly in local manpower, materials, and resources on a cooperative basis. In varying degrees, one wonders how much they contribute ideas, raw materials, and manpower to Japan.

The book is well documented with official speeches, official data, and interview records with government officials and the Liberal Democratic Party leaders. It is descriptive as well as quantitative in its analysis. It contains a bibliography of selected books and articles for further references. The book should be highly recommended since it analyzes many valid and important questions about Japan's "manner of giving."

PETER CHENG

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Erratum

Hayward R. Alker, Jr. "The Dialectical Logic of Thucydides' Melian Dialogue" (September 1988, 805–20). On page 811, col. 1, line 40 should read

$$\sim D/(N_1/\sim S_1 \ \& \ N_2/\sim S_2)\&!(N_1/\sim S_1 \ \& \ N_2/\sim S_2).$$

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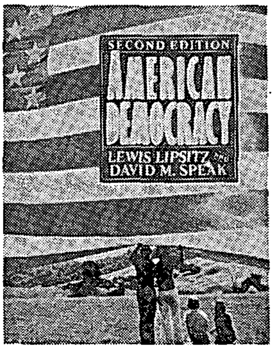

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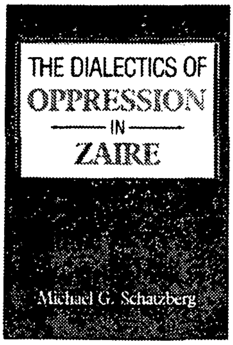
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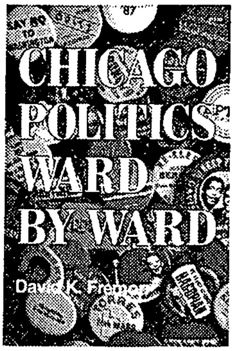
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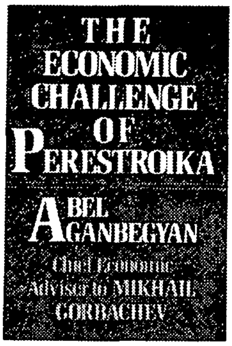
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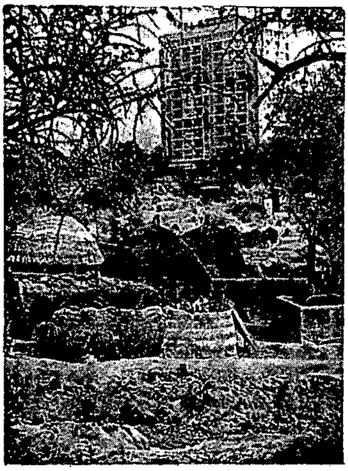

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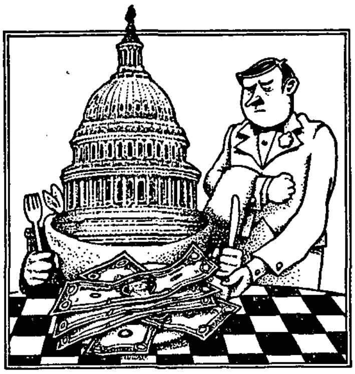

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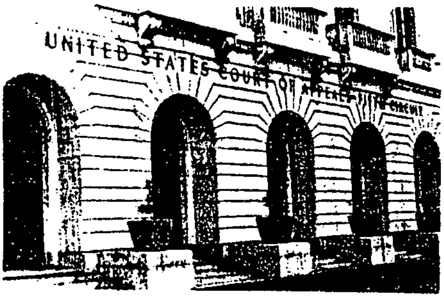

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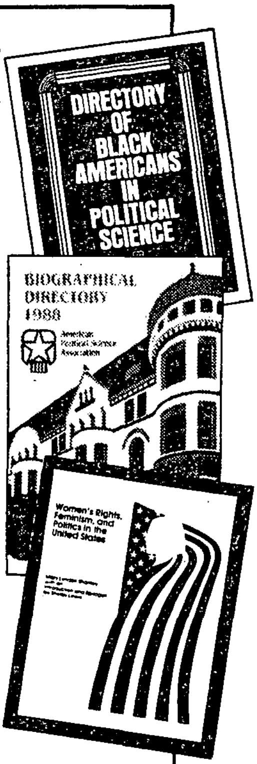
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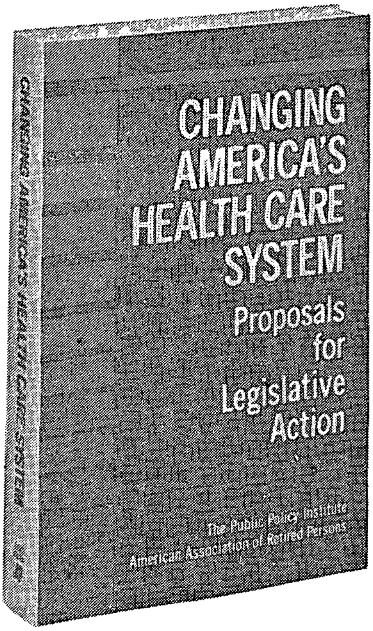
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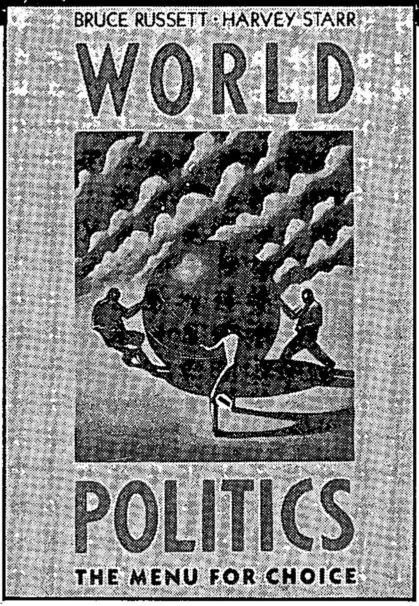

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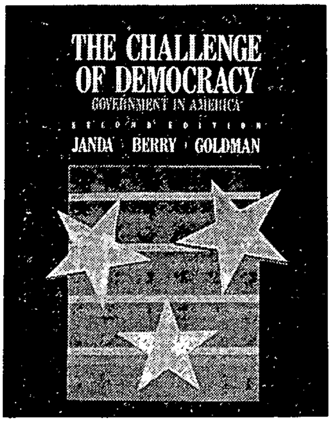

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